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ABSTRACT

This manual is a compilation of articles about women who pursue careers in science and the multiple sources and forms of discouragement they encounter. It is intended as a guide when developing courses that explore career opportunities and pressures for women in science and a resource in understanding the interaction between science, society, and individuals. Those who would find this manual useful would be postsecondary and secondary faculty and students of science, social science, and mathematics; career counseling personnel; and preservice and inservice teachers. An appendix provides references, career counseling information, and extensive resources for developing an introductory college course to encourage women to enter science fields. (Author/SA)

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Science Sex & Society

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SCIENCE, SEX AND SOCIETY

Science / Social Science
Postsecondary Mathematics
Career Counseling

What?

Women pursuing careers in science have faced multiple sources and forms of discouragement. Traditional patterns of sex-role stereotyping and sex-role socialization have historically been a barrier to women's scientific achievement. This collection of readings examines a multitude of issues relevant to the interrelationships among science, society, and the effects on the lives of women as scientists.

The readings in one unit compare the experiences of women in the past with contemporary women providing a perspective on the education of women in science. Another section explores the nature of science as a field of study and career choice. This particular section also examines some ways science influences society and, conversely, how biased beliefs of a society can shape individuals' lives. Other units focus on combining family and professional responsibilities and the process by which women choose careers. An extensive bibliography accompanies each unit.

An appendix provides additional references, career counseling information, and extensive resources for developing an introductory college course to encourage women to explore opportunities in science.

Special equipment needed: various exercises require materials commonly available in college science laboratories.

Who?

Postsecondary and secondary faculty
and students of science, social
science, and mathematics

Career counseling personnel

Preservice and inservice teachers

How?

This manual will serve as a guide
when developing courses that explore
career opportunities and pressures
for women in science and a resource
in understanding the interaction
between science, society, and
individuals.

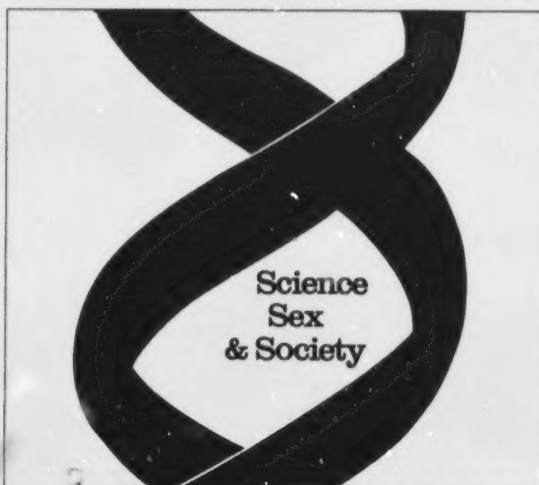
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Science, Sex, and Society

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Kansas State University
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The Project for the Advancement of Women
In Science Careers

Women's Educational Equity Act Program
U.S. DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE
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PREFACE

In preparing this book we have aimed at an ambitious and multifaceted goal: to illustrate the web of interrelationships among science, people, and society; to demystify science; to illuminate the influence of science on women; to show that scientists are human and that some of them are women; to examine the challenges facing a woman who wishes to be a scientist, wife, and mother; and to encourage young women with talents to use those talents.

Young women who have high potential for successful careers in science face multiple sources and forms of discouragement that may prevent them from fulfilling their potential. Sex-role stereotyping and sex-role socialization with regard to career choices begin early in childhood. As a consequence of this socialization, high school seniors who have aptitudes for science often assert that role conflict between being a professional and being a wife and mother is a barrier to women, and they resolve the conflict by choosing the wife and mother role. Studies such as the Carnegie Commission Report, Opportunities for Women in Higher Education, have identified three related barriers: (1) early acculturation that denies girls experience and the resulting skills in problem solving and the use of tools and mechanical objects; (2) the paucity of successful women scientists to serve as role models and the general male sex stereotyping of the scientific professions; and (3) sex-biased career counseling that directs high-ability young women into certain nonscience occupations thought to be more compatible with the traditional role of wife and mother. Clearly these problems must be approached at several levels in the educational system. At the college level we can help women students to overcome these barriers by providing: (1) experience in using tools and machines and in problem solving; (2) role models and information on the contributions of women in science; (3) unbiased career counseling and support from peers with similar interests; and (4) opportunities to evaluate their goals and life plans. In this book we have provided resource materials related to each of these means of encouraging women to enter science careers. We hope that these readings will encourage concerned but busy scientists to teach a course directed at reducing the barriers to the equal participation of women in science.

This book grew out of the collaborative efforts of many women. The original impetus came from the concern of our affirmative action officer, Dorothy Thompson, about the paucity of women faculty members in science. She encouraged Ann Kammer, neurobiologist and reluctant feminist, to develop a program that would encourage more women to choose careers in science.

Ann was joined by Jan Sloan, a microbiologist with a strong interest in the history of women in science, and Cherry Granrose, biologist and developmental psychologist with interests in women's careers and reproduction. Collaboration among the three of us stimulated development of an interdisciplinary project that was funded by the Women's Educational Equity Act Program.

Ann Kammer and Cherry Granrose were responsible for the final contents of the book. Ann was primarily responsible for Section III, The Nature of Science, and the laboratory exercises. Cherry developed Section IV, Lifestyles, and most of the Appendix. Jan Sloan, who provided an enriching historical perspective throughout the book, assembled Section II, Becoming a Scientist. The WEEAP grant enabled us to obtain the assistance of a graduate student, Kitt Shaffer, who compiled the bibliography of audio-visual materials and collected material for other bibliographies. Several undergraduate students also assisted in the library and office chores: Marla Porter, Claudia Kale, Pat Ekey, and Kallie Bernbeck. We would not have survived the final throes of manuscript preparation without the diligent labor and concern of Gloria Caffey. Three faculty members contributed information in their specialties: Dr. Janet Benson, Anthropology; Dr. Elizabeth Unger, Computer Science; and Dr. Donna Kater, Counseling. The monstrous chores of typing and retyping were ably performed by Sharon Martin, Craig Kreiser, Sharon Kay Franks, Lori Nighswonger, Debbie Sodamann, and Ann K. Ogborn. Illustrations were drawn by Jon Hudson.

Two groups of students in our course, SCIENCE, SEX, AND SOCIETY, shared with us their responses to the materials and so helped us to improve them. Many authors and publishers gave us permission to reprint their material. They are designated throughout the book. We are particularly appreciative of those who sent us moral support and encouragement along with their permission form. We are also grateful to the Women's Educational Equity Act Program of the Office of Education for financial support and for the opportunity to meet other people interested in improving the status of women.

We are interested in readers' reactions to these materials. Are they useful? Have we made major errors of fact or omission? Do you have better ideas that work? Let us know.

We hope this book will prompt some scientists and their students to examine the issues raised. More important, we hope the book will help young women make knowledgeable choices about science careers.

Gentle Reader:

We have compiled these articles to help you explore the terra incognita of women in science. Two main goals have guided our choice of articles to be included: (1) to encourage talented women to enter careers in science and to make full use of their talents; and (2) to enable citizens, scientists or not, to understand the interactions of science and society and, especially, to understand the effects these interactions can have on their own lives and on the lives of many women.

Science and society are abstractions; the reality is comprised of people and their actions. Therefore the book begins with people and the process by which they became scientists. In order to discover our heritage, we look at the lives of several women who became scientists during the past century. These women contributed to the scientific knowledge of their time, even though they often encountered barriers to obtaining the necessary education. Comparing the experiences of women in the past with those of contemporary women provides us with a perspective on the education of women scientists today.

In the next section we consider the nature of science, the methods used by scientists, and the social aspects of their activities. The methods scientists use are exemplified by the discovery of the structure of DNA. The social interactions between scientists and their society are illustrated by scientific research on women. Science and society are woven together in a complicated fabric of continually changing forces and restraints. Because science and society are inextricably interwoven, activity in one influences the course of the other. Understanding this relationship is important for both scientists and nonscientists, because such knowledge allows the educated person to live the examined life. Living the examined life entails both informed contemplation of talents, values, and opportunities, as well as deliberate action to fulfill individual aspirations. To further this process, we provide examples of various lifestyles of women scientists. In Section III, we address a question that has been considered particularly important for women, namely, how best to combine family and professional responsibilities. The experiences of many individuals are drawn together here to provide a basis for evaluating the consequences of choosing traditional or innovative ways of answering this question. We also consider the process by which women choose careers and provide information to assist the reader in making enlightened choices.

The Appendices contain additional references, lists of possible resources, and teaching materials. We have included this information primarily to aid faculty members in teaching a course like ours. The interested student may also find them useful.

One of the important points we wish to make throughout this book is that science, despite its reputation, is not objective knowledge and that scientists are unavoidably biased by their culture. Bias is a problem with which we, too, have had to cope in writing this book. We have attempted to counter our bias as white academics by including articles about the experiences of minority and nonacademic women. (A forthcoming Annals of the New York Academy of Science will provide additional information on minority women and women who work in industry.)

A major bias that we have not tried to avoid comes from our experiences as women. It is our belief that women's experiences contribute a valuable perspective, worthy of examination. Women almost inevitably bring to science experiences different from those of men, because boys and girls are socialized differently. These differences are important, because some people can, out of ordinary experiences, make extraordinary observations. To the extent that certain kinds of ordinary experiences are commonly female, observant women can make unique contributions. On the other hand, exclusion of females from ordinary experiences of males has, in some cases, limited women's participation in science. We hope these readings will encourage women to value those different experiences that allow them to make unique observations and to recognize and reduce those differences that raise barriers to the full participation of women in science.

Because most scientists have been men, the value system of science has been dominated by the male point of view. The male value system, in turn, influences the choice of problems to be investigated, the methods used, the interpretation of data, and the application of results to the progress of society. Women who become scientists either accept the male value system or deal with the difficulty that "different" is commonly equated with "inferior." As more women become scientists, this "difference" will diminish. At this moment, however, the prevailing intellectual climate both requires and allows us to examine that which is peculiar to women. Scientists do not know enough about female experiences to predict what changes will ensue in the value system and the progress of science when more women participate, nor do we know enough about the best ways to facilitate the contributions of women in science. To enhance women's participation, both better understanding and action are needed. We hope this book introduces you to the realities of the present and the potential for change in the future.

This book may be more useful to you if you read it with questions in mind. We suggest that you formulate a list of your own questions about women in science and then look through the book to see where (and if) there are some answers. The questions that we think important are these: What does a

working scientist do? What factors influence people to become scientists? During the last 200 years, how have ideas of women's roles changed to include a scientific profession? What contributions have women scientists made? What are the rewards of being a scientist? What lifestyles are compatible with being a successful scientist? How does a person choose a career, and what are the options for careers in science? Keep your questions and ours in mind as you read and as you encounter scientists and their work.

Maria
Mitchell



Becoming a Scientist

Education: A Historical View

Woman's ability to contribute to scientific knowledge is inseparably intertwined with her ability to have access to a scientific education. Historically, the inaccessibility of educational opportunities has been a great barrier to women's scientific achievement and may explain, in part, why no woman has ever achieved the scientific stature of Einstein or Newton.

Before the educational process became institutionalized, there were several ways a person of sufficient economic status could obtain scientific knowledge: by training at a religious institution, by self-education, or through tutelage in the family home either by relatives or hired tutors. Examples exist of women who were educated for scientific pursuits by each of these methods; however, men undoubtedly had easier access to these educational processes because of the prevailing societal view that woman's place was in the home. When education became the function of schools, colleges, and universities, women continued to encounter barriers to obtaining a formal education. Access is certainly easier to obtain today than it was 100 or even 50 years ago, and the problems that persist are more in the realm of attitudes than of written regulations.

The articles in this section provide a historical overview of scientific education for women. Consider the following questions as you read:

- 1) How did women reconcile their scientific interests with the societal expectations for women of their day?
- 2) What influence did the families of women have on their educational experiences? Were people other than relatives important?
- 3) Have the fields of science entered by women changed through history?
- 4) Do you think women today make the best possible use of their educational opportunities? If not, what changes need to be made to utilize fully the untapped scientific potential of women?
- 5) Do you think women are taken seriously during their scientific training and as professional scientists?

- 6) What has been your personal experience in education? Examine the reception of your family, teachers, and friends to your own personal interest in science or other field of study.

Family Influences

Mary Somerville (1780-1872) predicted the existence of Neptune and discovered the basic relationships that were later called the principle of the conservation of energy. This biography describes her self-acquired education, as well as the unfavorable attitude her parents and first husband expressed regarding her scientific interests. The constraints she experienced during her first marriage contrast with the intellectual freedom she experienced during her widowhood. An even greater contrast can be observed with respect to her second husband's encouragement and support of her scientific endeavors.

MARY SOMERVILLE:

A NEGLECTED POPULARIZER OF SCIENCE

by George Basalla

This first appeared in New Scientist, London, The Weekly Review of Science and Technology 329 (March 7, 1963): 531-533.

The Victorian era was a time of great popular interest in science. There were lecture demonstrations illustrating the latest discoveries, books and compendiums of scientific information, and the superb popularizations of men like Thomas Henry Huxley and John Tyndall who were intimately acquainted with current scientific developments. But while reviewing the popular science of Victoria's reign, we tend to neglect a figure whose accomplishments dominated the earlier decades of the reign, and whose sex marked her as a unique individual in a society and a field of study ruled by males. Yet in her day, Mrs. Mary Fairfax Somerville enjoyed the fame, if not the notoriety, that Huxley was to know in the latter part of the century.

Mrs. Somerville's works of popularization won her the friendship and respect of leading British and Continental scientists. She was a close friend of the astronomer Sir John Herschel and the geologist Sir Charles Lyell, and counted Laplace, Cuvier, Humboldt, and Ampère among her colleagues across the Channel. British, French, German, Italian, Swiss and American scientific societies offered her honorary memberships and although she was never to enter the exclusively masculine domains of the Royal Society her marble bust was placed in its rooms, alongside the statues of Sir Isaac Newton and Sir Humphry Davy. Even the Government recognized her talents. Sir Robert Peel bestowed one of the earliest scientific pensions upon Mrs. Somerville in 1835 and Lord John Russell, hearing of her financial needs, increased its value when he took office some ten years later. Finally, there are few scientists who can claim this honour: a sailing vessel destined for the China trade was named Mary Somerville and it carried a copy of her bust as its figurehead!

Mrs. Somerville made her debut as a popular writer on scientific subjects in 1831 when she published Mechanism of the Heavens. This book presented the first comprehensive exposition in the English language of the Marquis de Laplace's great treatise on physical astronomy: Mécanique céleste. Laplace had successfully completed the search begun by Newton in the seventeenth century for a physico-mathematical theory of such breadth and accuracy that it could account for the motions and positions of all the major astronomical bodies in our solar system. The last of the five volumes of Laplace's treatise appeared in 1825 and the whole enterprise was met with much the same awe that greeted the announcement of Einstein's theories in the early twentieth century. It was confidently stated that there were but twenty men in France, and perhaps half that number in England, who could read and understand the French astronomer. The Mécanique céleste immediately became a touchstone for the recognition of superior intellect and mathematical talent. Mrs. Somerville, as the translator and expositor of this abstruse work, naturally received the praise of the scientist and the admiration of the layman for her labours. To these accolades could be added Laplace's comment that she was the only woman fully to comprehend his Mécanique céleste.

Despite Mrs. Somerville's modest claim that she "simply translated Laplace's work from algebra into common language," the mathematically unsophisticated reader was surprised to find that nearly every page of the Mechanism of the Heavens was filled with equations. Laplace had been condensed and simplified but the calculus was still necessary as a tool for studying physical astronomy. The high mathematical level of the book made it an unsuitable candidate for its original intention, as a contribution to Lord Brougham's popular scientific series, and it limited the volume's general sale.

Mrs. Somerville's Laplace did not receive wide distribution but it gained the higher honour of being recommended by two Cambridge University mathematicians (George Peacock and the Reverend William Whewell) to those of their students "who aspire to the highest places in the examinations." With this recommendation, virtually the whole printing of 750 copies was sold in Cambridge. Meanwhile, the less knowledgeable elements of the British and American public could read the nonmathematical "Preliminary Discourse" which had prefaced the original edition but was soon printed as a separate essay.

Mrs. Somerville's second attempt at the simplification and explanation of science was a much greater popular success than her interpretation of Laplace. The Connexion of the Physical Sciences, published in 1834, went through nine English editions and achieved the distinction of being translated into German and Italian, and pirated by an American publisher. In this work, the reader was first introduced to the rudiments of physical astronomy and then presented a survey of recent scientific research. Topics in dynamics, the theory of matter, pneumatics, sound, optics, heat, climatology, electricity, magnetism, chemistry, and stellar astronomy were drawn together to produce a unified picture of the physical universe. It was possible to effect smooth transitions between these disparate topics

because Mrs. Somerville strove to show that there were indeed "connexions" between what had hitherto been considered autonomous fields of inquiry.

In modern times the principle of the conservation of energy would be invoked to account for the "connexions" Mrs. Somerville found in the conversion processes relating magnetism, electricity, and chemistry, or heat and mechanical work. However, in 1834 that principle had not yet been enunciated and the author of The Connexion of the Physical Sciences exhibited the depth of her understanding of these recently discovered conversion processes when she utilized them to call attention to the multiple interrelationships existing between apparently diverse physical phenomena. All of this is not to say that Mary Somerville was the "true" discoverer of the energy conservation principle. A more historically accurate appraisal would stress the fact that, at a critical time in the development of physical thought, she was one of the first to perceive the significance of the related conversion processes.

If the search for "connexions" was the first step in the direction of the formulation of the principle of the conservation of energy, some other information contained in The Connexion of the Physical Sciences led directly to another important scientific discovery. In preparing the third edition of her book, Mrs. Somerville reviewed the latest monographs discussing planetary theory, studied the question of the irregularities in the motion of the planet Uranus, and announced that these irregularities might be due to the influence of some unknown and "unseen planet revolving about the Sun beyond the present boundaries of our system." At Cambridge, the young mathematician John Couch Adams read her explanation for the perturbations of Uranus and began his mathematical reconstruction of the orbit the hypothetical planet must traverse in order to produce its observed effect upon Uranus. Adams's investigation led him to confirm the Somerville hypothesis and to predict the existence of the planet Neptune as the body disturbing the motions of Uranus. This theoretically determined prediction of the position and orbit of a planet not yet seen by the astronomers marks one of the high points in nineteenth century astronomy.

What was the educational background of a woman who could point the way to the discovery of a new planet, adumbrate the principle of energy conservation, and translate a difficult work on celestial mechanics? As might be expected, the answer is that in large measure she was self-taught. During the first half of the nineteenth century there was scarcely any provision made for men interested in a scientific education and certainly none for the rare female who sought such training. Given these circumstances, it is not surprising to learn that Mrs. Somerville was to spend many years in slowly acquiring the knowledge she would use so effectively in her later life.

Mary Fairfax Somerville was born in Jedburgh, Roxburghshire, Scotland, on December 26, 1780, to Sir William George Fairfax, an officer in the Royal Navy, and his wife Margaret. Like most of the other girls in town, Mary received no formal education before she was ten. At that time she was sent to Miss Primrose's, a local boarding school, where her father hoped she would learn to write well and keep household accounts. Miss

Primrose taught some rudiments of the French language to her students but she offered them no scientific subjects. The young Miss Fairfax compensated for the school's inadequacies, and fulfilled her intuitive, passionate interest in the natural world, by observing marine animals, botanizing in the surrounding fields, gazing at the stars from her bedroom window, and collecting shells, birds' eggs, and rocks. At the age of fourteen or fifteen she discovered algebra, in the form of a mathematical puzzle printed in a monthly ladies' magazine, and resolved to learn the meaning of the "strange looking lines mixed with letters, chiefly Xs and Ys." Robertson's Navigation, a volume available in the nautical Fairfax household, did not help Mary in her search for the key to algebraic equations but her painting teacher did when he recommended that she begin her mathematical education with a reading of Euclid's Elements of Geometry. Euclid and a copy of Bonnycastle's Algebra were smuggled to the young woman by a sympathetic family tutor, and she studied these books, along with Greek and Latin grammar, late at night when she could remove herself from the critical surveillance of her parents.

This slow and furtive process of education was all but halted in 1804 with the marriage of Miss Fairfax to Samuel Greig, a man who, in her words: "had a very low opinion of the capacity of my sex, and had neither knowledge of nor interest in science." After three years of married life in London, her husband died and she returned to Scotland a widow with two small children. The lonely, quiet years of widowhood gave her ample time to renew her mathematical studies. She had already learned plane and spherical trigonometry, as well as conic sections, and she now turned to Sir Isaac Newton's formidable Principia. Once again her studies were interrupted by marriage, this time to Dr. William Somerville, but now she had found a spouse who approved of her scientific interests. In 1813, a year after her second marriage, Mrs. Somerville finally began an exhaustive study of Lacroix, Biot, Lagrange, Clairaut, Laplace, and the other French mathematicians and astronomers who had written the advanced texts of the day. With the encouragement of her husband, she became personally acquainted with the major British scientists (Thomas Young, William H. Wollaston, Michael Faraday, William Buckland) and found herself recognized as an equal in their midst. Their recognition held a new meaning after 1826, when she formally entered the circle of contributing scientists with the publication of an article in the Philosophical Transactions of the Royal Society on the supposed magnetizing influence of the violet rays of the solar spectrum.

Although the mathematical sciences held a predominant place in her life, Mrs. Somerville was never so narrowly dedicated to them that she disregarded activities in the social, aesthetic, and intellectual world. Her lack of pedantry, and her essential femininity, were defended by no less a figure than Lord Francis Jeffrey of the Edinburgh Review. In replying to a friend who characterized Mary Somerville as a bluestocking, he remarked: "The lady of whom you speak may wear blue stockings, but her petticoats are so long that I have never seen them." As a young woman she was popular at the balls, teas, supper parties, and theatrical performances which constituted the gay social life of Edinburgh. This popularity was founded upon her physical attractiveness and upon her accomplishments in

music and the fine arts. She held a lifelong interest in literature. Sir Walter Scott and Maria Edgeworth were her close friends--and, with Dr. Somerville, she travelled extensively throughout Europe meeting many of the brilliant French and Italian artistic personages of the century.

It was while Mrs. Somerville was on one of these extended Continental tours that she began work on her third volume of popular science. Her previous book had gained its unity through the search for "connexions" between the physical sciences. The new volume, entitled Physical Geography, was to achieve a natural unity by its discussion of the organic and inorganic sciences immediately associated with the Earth, its organisms and its atmosphere. Geology, topography, hydrography, meteorology, oceanography, and geographical botany and zoology were some of the sciences the author summarized as she turned her reader's attention to an area of study which had received scant treatment in England prior to the appearance of her survey in 1848.

The Physical Geography appeared simultaneously with a work of a similar nature but much grander in scope: Alexander von Humboldt's Cosmos. Mrs. Somerville believed her review of physical geography would suffer in comparison with Humboldt's masterpiece, but the critics were very kind and Humboldt augmented their praises by personally commenting: "I do not know of a work on physical geography in any language than can compare with yours." Once again Mrs. Somerville had produced a popular and critical success. There were the usual English editions and the translations into foreign languages plus the additional distinction of having the volume used as a textbook in as diverse places as the Military Academy at Sandhurst and the government schools in Bengal. This unique British contribution to the new science of physical geography received its highest award in 1869 when its author was presented with the Victoria Medal of the Royal Geographical Society. Lady Franklin, the wife of the missing Arctic explorer Sir John Franklin, was the only female to precede Mrs. Somerville in the receipt of this medal.

Mrs. Somerville was 68 years old when the Physical Geography was released to the public but she had no intention of retiring from the writing of popular science. There was still much work to be done. She periodically revised the old editions of her books so that they would reflect current scientific thought, pursued her study of mathematics and physics, and continued her long journeys to the Continent. In 1861, a year after the death of her husband, she undertook research for a two-volume treatise that was to be completed on the eve of her 90th birthday: Molecular and Microscopic Science (1869). The size of the particle, molecular in the physical sciences, and microscopic in the biological ones, determined the subject matter of the new work and gave it coherency. Perhaps it was the arbitrary choice of size as the dominating factor in selecting material, or perhaps it was simply that Mrs. Somerville had lost her powers of popularization; in any case, Molecular and Microscopic Science won more respect as the production of a nonagenarian than it did as a contribution to the literature of popular science. Three years after the publication of this book Mrs. Somerville died in Naples, Italy. In typical fashion her last years were spent in reading some mathematical texts she had received: notably

William R. Hamilton's Lectures on Quaternions and the American Benjamin Pierce's Linear Associative Algebra.

Mrs. Somerville should be remembered both for her substantial achievements in the field of popular science and for her determination to obtain a scientific education in the face of opposing family and social pressures. As a popularizer of science she possessed the ability to cover a great range of topics and yet make them comprehensible to the layman. To this genius for simplification should be added her profound insight into those ideas that stood on the advancing edge of science. There have been few, if any, popular writers who have equalled her skill in surveying recent scientific developments and then indicating novel syntheses of experimental data and new directions for future investigations.

It is inspiring to know that these achievements were the result of one woman's single-minded determination to learn all she could about science despite the prejudices of her culture. In view of the difficulties she surmounted in obtaining her education, it is certainly appropriate that within six years of Mrs. Somerville's death Oxford University should name one of its first women's colleges in her memory. She deserved this posthumous honour, as well as the many tributes she gained during her lifetime, for she was undoubtedly one of the greatest women scientists Britain ever produced. Yet, might we not have been able to record still greater scientific contributions had her youthful interests, mathematical genius, and creative imagination been developed in a more favorable educational environment?

Author's Note: For additional reading, the best biographical source is Personal Recollections of Mary Somerville: With Selections from Her Correspondence, a book written and edited by her daughter, Martha Somerville. There is no comprehensive modern appraisal of Mrs. Somerville's achievements; however, Thomas S. Kuhn, "Energy Conservation as an Example of Simultaneous Discovery" (printed in Critical Problems in the History of Science, 1959, ed. by Marshall Clagett), and Morton Grosser, The Discovery of Neptune, 1962, both discuss her influence on their respective topics. All of these are to be supplemented by the reviews and notices of Mrs. Somerville's books appearing in Victorian periodical literature.

The following account of the education Jane Colden (1724-1766) received from her father Cadwallader Colden illustrates both the importance of education and the attitudes surrounding its provision to women during colonial times. In spite of limited access to educational opportunities, Jane Colden and other women made significant contributions in botany and agriculture in colonial America.

DANCING DOGS OF THE COLONIAL PERIOD: WOMEN SCIENTISTS

by Joan Hoff Wilson

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Except for books written by women, historians of science have traditionally ignored the contributions of women scientists or so defined modern science as to exclude them. (1) Nowhere is this more evident than during the colonial period in the natural sciences. As medical practitioners in an era of all too few trained physicians, numerous, historically obscure women performed with apparently unlimited freedom the practice of "physick and chirurgery" because it was thought that they had natural healing abilities. And of course they had a monopoly on the equally unacknowledged, if not always untrained, profession of midwifery. (2) But even in the fields of agronomy and botany, where individual colonial women did excel, their historical and scientific fate has been one of neglect for the following reasons. (3)

Like the male agronomists and botanists before the American Revolution, only upper-class women had the opportunity to delve into these two related sciences. Agronomy in the seventeenth and early eighteenth centuries represented a transformation of medieval agricultural practices by the systematic application of soil and plant knowledge to soil management and the raising of crops. This development marked the birth of scientific agriculture. At the same time botany underwent a revival, especially in the first half of the eighteenth century, under the influence of the new classification system of Linnaeus. This renewed interest often took the form of botanical gardens and private collections in wealthy homes--many of which later became museums.

Jane Colden Farquher (1724-1766) came from one such aristocratic colonial home. Her father, Cadwallader Colden (1688-1776), sailed to the New World in 1710 trained in medicine but soon turned to public service. He was Lt. Governor of New York from 1761 until his death, and a long-time member of the Council of New York and Surveyor General for New York. His far-flung scientific interests, however, including his personal correspondence with Linnaeus, and his conviction that women were particularly suited for the study of botany led him to educate and encourage his daughter to work in this field. (4) While his close relationship with her

and his concern for her education is in keeping with what Page Smith has called the "potency of daughterhood," Colden's actions and attitudes were less free of sex-stereotyping than is usually implied in the standard accounts of Jane Colden's achievements as a botanist, (5) and Smith's term. (6)

One of the reasons for this was the fact that Colden thought women should study botany because of "their natural curiosity and the pleasure they take in the beauty and variety of dress seems to fit them for it." Moreover, he viewed such study as an ideal substitute for idleness among his female children when he moved his family to the country in 1729. As such it was "an Amusement which may be made agreeable for the Ladies who are often at a loss to fill their time...." He went so far as to recommend that perhaps from Jane's example "young ladies in a like situation may find an agreeable way to fill up some part of their time which otherwise might be heavy on their hand. May amuse and please themselves and at the same time be useful to others. (7)

Despite what he thought to be their sex-determined propensity for botany, Colden realized that it was "not usual for women to take pleasure in Botany as a Science...." Thus, he went out of his way to translate Linnaeus' binomial method from Latin for his daughter not only to fill up her otherwise wasted time with a worthwhile activity, but also because he apparently did not think that Jane and other women were capable of learning the language. "The chief reason that few or none of them have hitherto applied themselves to this study," he explained in 1755, "is because all the books of any value are wrote in Latin and so filled with technical words that the obtaining the necessary previous knowledge is so tiresome and disagreeable that they are discouraged at the first setting out and give it over before they can receive any pleasure in the pursuit. (8)

Colden's private letters clearly reveal his conviction that he literally had to trick his daughter into studying the subject even though she had an "inclination to reading and a curiosity for natural phylosophy or natural History and a sufficient capacity for attaining a competent Knowledge." So he translated the Linnaean system into English "by freeing it from the Technical terms which was easily done by using two or three words in the place of one." By 1755 he was also convinced that "tho' perhaps she could not have been persuaded to learn the terms at first she now understands in some degree Linnaeus' characters notwithstanding that she does not understand Latin." Several years later he reiterated to Dr. John Fothergill how Jane had "eagerly swallow'd the bait" once he had presented her with the simplified translation and how "her fondness for the amusement made her acquire some Knowledge of botanic latin" without otherwise understanding "anything of the language. (9)

Obviously in an age when much scientific work was still reported in the universal language of Latin, Jane Colden's lack of training in it was a basic inhibiting factor for her career as the lack of formal education, which it represented, was for all women in the sciences and other professions. This potentially debilitating feature of the educational experience of colonial women emerged full-blown after the American Revolution as professional

training instead of the apprenticeship method became more popular in the United States as a means for training all professional people. (10)

Hence, even though Jane Colden discovered a new genus, she was unable to communicate her findings directly to the scientific communities at home or abroad. This was done for her by her father or by his botanical friends like Alexander Garden and Peter Collinson. They even offered her services to other naturalists and botanists in this indirect fashion. In the few cases when Jane did correspond on her own, her letters were usually answered through her father. Therefore, far from being regarded and treated as a professional botanist by her father and his friends, she appeared in their letters (like Samuel Johnson's dancing dog) to amaze them, not for what she had accomplished, but that she did anything at all in this field. (11)

For example, Collinson reported to Colden in 1757 that his "ingenious" daughter was "the Only Lady that I have yet heard off that is a professor (of) the Linnaean System" and that Linnaeus was duly proud of this fact. Similarly, the most complimentary statements about her scientific abilities were undercut by a sense of absolute amazement and incredulity. Perhaps even worse, in terms of her professional acceptance, were comments about her womanly ways, such as what good cheese she made, in order "to palliate and excuse so much feminine scientific knowledge." (12)

Less conclusive but nonetheless suggestive evidence indicates that in general the same fate befell other outstanding women in scientific professions of the colonial period despite, or perhaps because of, the patriarchal sexism which permeated even those households where the "potency of daughterhood" prevailed. Not only did these other distinguished women scientists almost always come from wealthy families or obtain wealth through marriage or by inheritance, most of them (and their male colleagues) also justified their scientific activities as a sure means to overcome the peculiarly female vice of idleness (13) rather than in the name of science and progress. At best their activities were defended by themselves and others for their immediate and thus limited beneficial effects, which usually turned out to be medicinal or pecuniary. Most important, none of the colonial women scientists about whom we currently have knowledge dared to go into fields considered improper female spheres of activity; nor were they encouraged to do so by their fathers, husbands, or other male members of their families, however supportive they otherwise might have been in their careers. (14)

As a result of these sex-determined restrictions, the entirely new addition to science in the first half of the eighteenth century which created such excitement and spectacular experiments--electricity--was not deemed a proper field of study for women. Accordingly, Colden encouraged his sons to experiment with electricity, but not his daughters. It was not that experimentation per se was a forbidden activity for women, but there was a strong tendency to emphasize the medicinal or other practical housewifery use of the end products of their scientific endeavors to which the fields of agronomy, botany, horticulture and the "practice of physic" within individual households best lent themselves. (15)

Given the harshness of life in the colonies, even among the wealthy, this pragmatic approach to science was a common trait of male experimenters and inventors as well, but it appears particularly pronounced and hence restricting with respect to women because of the limited number of acceptable scientific careers open to them, especially in the new fields which would later attract most of the attention of historians of science. Furthermore, the connection between botany and medicine was a traditional and natural one before Linnaeus because doctors had dominated botanical work with their herb gardens from which they prepared physic. After Linnaeus had enunciated his principles, however, the earlier relationship between botany and medicine remained a prerequisite for women scientists in either field when this was increasingly less so for men. Thus, Jane Colden's three hundred or more plant descriptions and drawings were praised as much for the fact that many were "found useful in Medicine," as they were for their accuracy and beauty and strict adherence to Linnaeus' uniform binomial system for naming them. (16)

The same traditional medicinal overtones continued to be found among the wealthy women landowners who as agronomists and horticulturists conducted agricultural experiments while their male counterparts moved more quickly into newer applications of scientific agriculture. For example, Sibilla Masters, the first American inventor to receive an English patent (through her husband) for a new method of cleaning and curing Indian corn, (17) strongly recommended this "Tuscarora Rice" for the "relief and recovery of consumptive and sickly persons." The truly lasting and significant value of her invention turned out to be the water mill built by her husband Thomas Masters near Philadelphia to cure quantities of Indian corn for profitable sale. (18)

In retrospect, therefore, the connection between new and revitalized science and the Industrial Revolution of the eighteenth and nineteenth centuries is obvious, as is the fact that male, rather than female, scientists were initially instrumental in forging this bond and bringing about the "radical change in the kind of relations between science and economic life" so characteristic of the modern world. (19) From this standpoint it is understandable that historians of science have neglected the experiments of the horticulturist Martha Laurens Ramsay (1759-1811) which focused on the growing and preserving of olives in South Carolina despite the success of her efforts. A similar fate was accorded another colonial horticulturist, Martha Daniell Logan (1702-1779), whose posthumously published book, The Garden's Kalendar, served as the standard work on gardening for upper South Carolina in the last quarter of the eighteenth century. (20)

Even the most noteworthy of all the female agricultural experimenters, agronomist Eliza Lucas Pinckney (1723-1793), has suffered from neglect in the hands of historians of science despite her perfection of the cultivation of indigo. This crop provided the colony of South Carolina with its major, staple export from 1744 until the American Revolution. She also experimented less profitably, but with some success, at crop rotation, intensive farming, grafting, and general improvement of stock and seed from indigo to figs. But silver mining and certain unwomanly scientific pursuits she candidly admitted were beyond the range of her "fertile brain at

schemeing." "I love the vegitable world extreamly," she wrote in 1742 at the age of 19; "I think it an innocent and useful amusement. . . ." It prevented her from sinking into the "lethargy of stupidity. (21)

That these and other scientific botanists, agronomists, and horticulturists functioned as experimental scientists of their time is unquestionable. But except for those few who, like Eliza Pinckney, left elaborate records of their activities, what we know of their scientific careers has been related by men either through their private letters, or in the case of Martha Ramsay, through an account of her life by her husband David Ramsay. This indirect process of historical preservation itself has resulted, with the passage of time, in minimizing or ignoring the achievements of colonial women scientists. Their ultimate neglect by historians of science, however, has been primarily due to the fact that their scientific activities did not usually fall into the exciting, new categories of modern science which emerged before the American Revolution, and to the fact that they remained dancing dogs even in the more traditional scientific fields.

If this was true of the most prominent women scientists, it was all the more true of the largely anonymous group of family "doctresses," midwives, nurses and faith healers. Usually lacking formal education of any kind this vast group of women healers in colonial America and abroad were increasingly and relatively "easily put down as ignorant 'old wives' clinging to the superstitions of the past." Except for nurses, women were finally replaced in the course of the nineteenth century by a male medical profession which "was distinguished not so much by its associations with modern science as by its associations with the emerging American business establishment. (22) Rather than chauvinist conspiracy by male historians, the historical neglect of both types of women scientists--the solo dancers as well as the faceless chorus line--reflects these complex developments in science, industry and the American economy in general following 1776 plus the greater sex-stereotyping of political and socio-economic roles which occurred following the Revolution. (23) The neglect by historians of literature of the books and letters written by Jane Colden, Martha Logan and others reflects double banishment--banishment of women from science and science from literature.

There is one final reason for this neglect of colonial women scientists as individuals and as professional or semi-skilled groups. It lies in the general inadequacy of historical analysis in the entire field of women's history. Anyone who has read extensively in the history of American women or who has attempted to teach classes based on such material is usually struck by the lack of systematic analysis and the repetitious reliance upon the same documents and anecdotes which characterize the standard secondary works in this field. (24)

Past and present studies, by feminists and sexists alike, have either emphasized the total invisibility of subjugated American women or the exceptional appearance from time to time of particular women in science or other professions: Colden, Pinckney, Logan, in science; Bradstreet, Rowlandson, Knight, in letters. Both approaches are inadequate. The former is obviously contradicted by the existence of the exceptions, while

the latter makes the measure or standards for determining what is important about women in history the same as those which currently exist for men. The result has been the use of power and prestige, prerequisites which have kept women out of most of standard history and literature textbooks as well as those on the history of science.

In some instances these two antithetical historical methods have been used alternately in monographs and in classes, which further contributes to internal contradictions and confusing historical analysis. Hence, a comprehensive conceptual framework is needed, not only by those interested in teaching and writing in the field of women's history, but by those in the increasingly popular and broad field of women's studies. And of course not only those interested primarily in women but as well by all students of early American culture.

Historians of science and of literature are in a particularly opportune position, therefore, to contribute to our knowledge of the significance of women in all aspects of American life, if they can recognize and overcome the current analytical inadequacies within their respective fields of history as they relate to women--if they can avoid, in other words, the exaggerations associated with the "total subjection" theory and, at the same time, refrain from singling out only those few female "exceptions" who have acted most like their male contemporaries. One of the best ways to avoid both pitfalls is to employ the method of multi-role analysis or to use what sociologists call a "structural-functional" approach to the study of American women as scientists, as writers, as anything else; that is, to study women in terms of societal processes rather than as isolated objects in a man-made world. To date this has not been done and the resulting methodological deficiencies and interpretive contradictions are most evident in the history of science where the dancing dog syndrome remains inviolate.

Footnotes

¹The following works are the standard ones by women which contain references to the scientific (including medical) activities of women during the colonial period: Mary Sumner Benson, Women in Eighteenth-Century America: A Study of Opinion and Social Usage (New York, 1935); Elisabeth Anthony Dexter, Colonial Women of Affairs: Women in Business and the Professions in America before 1776, 2nd ed., rev. (Boston, 1931); Alice Morse Earle, Colonial Dames and Good Wives (New York, 1895, rep. 1962); Barbara Ehrenreich and Deirdre English, Witches, Midwives and Nurses--A History of Women Healers (Glass Mountain Pamphlets, 1972); Alice Clark, Working Life of Women in the 17th Century (London, 1919); Josephine Herbst, New Green World (New York, 1954); Harriott R. Holbrook, Journal and Letters of Eliza Lucas Pinckney (Wormslow, Georgia, 1850); Mabel C. Holman, The Western Neck (Hartford, Conn., 1930); Julia Ward Howe, Woman's Work in America (New York, 1891); Kate Campbell Hurd-Mead, A History of Women in Medicine (Haddam, Conn., 1933); Eugenie Andruss Leonard, The Dear-Bought Heritage (Philadelphia, 1965); Melina Lepinska, Les Femmes et le Progrès des Sciences Médicales (Paris, 1930); Adelaide

Nutting and Lavinia Dock, A History of Nursing--From the Earliest Times to the Present Day, 4 vols. (New York, 1907-1912); Harriott H. Ravenel, Charleston: The Place and the People (New York, 1906); idem., Eliza Lucas Pinckney (New York, 1896); Julia Cherry Spruill, Women's Life and Work in the Southern Colonies (New York, 1938, rep. 1969); May (King) Van Rensselaer, The Goede Vrouw of Mana-ha-ta (New York, 1898).

²Spruill, 267, 272-75; Howe, 140-42; Leonard, 215-19; James R. Chadwick, The Study and Practice of Medicine by Women (New York, 1878), 445-46; James Anderson, Memorable Women of the Puritan Times (London, 1862), vol. II, 54-107; Dexter, 58-77; Ehrenreich and English, 18-21.

³Colonial women agronomists and horticulturists included: Frances Ann Tasker Carter (1737-1797); Affra Harleston Coming (d. 1699); Elizabeth Digges (d. 1699); Judith Giton Royer Manigault (d. 1711); Eliza Lucas Pinckney (1723-1793); Martha Laurens Ramsay (1759-1811). Colonial women botanists included: Jane Colden Farquher (1724-1766); Maria Drayton; Martha Daniell Logan (1702-1779).

⁴Alice Mapelsden Keys, Cadwallader Colden: A Representative Eighteenth Century Official (New York, 1967), pp. 1, 24-25; The Letters and Papers of Cadwallader Colden, vol. I (New York, 1918), i-x; vol. V; 1755-1760 (1923), 31, 216-17.

⁵Dexter, 118-19; Earle, 86-87; idem., Colonial Days in Old New York (Port Washington, New York, 1896, rep. 1962), 164-66; Herbst, 143; Van Rensselaer, 258-59.

⁶Page Smith, Daughters of the Promised Land: Women in American History (Boston, 1970), 39-40, 46-47. For a criticism of Smith's book, especially his attitude towards women and his theory of the "potency of daughterhood" (which is based on only six examples and which neglects all those women who apparently resisted their fathers' attempts to "emancipate" them in the colonial period such as Jane Colden's sister Elizabeth), see Lois W. Banner, "On Writing Women's History," Journal of Interdisciplinary History, II (1971), 356-57, and Linda Gordon et al., "Historical Phallicies: Sexism in American Historical Writing" (dittoed, expanded version of a paper delivered at the Dec. 1970 meeting of the American Historical Association).

⁷Letters and Papers, vol. V, 29-30, 39, 203, 217; Key, 6.

⁸Letters and Papers, vol. V, 29-30, 37.

⁹Ibid., 30, 203.

¹⁰For the general deficiencies in education for colonial women and the increasingly ornamental rather than utilitarian aspects of education for women after the American Revolution see: Spruill, 207, 231; Gerda Lerner, "Woman's Rights and American Feminism," American Scholar, XL (1971), 238-39; Benson, 157-61; Eleanor Flexner, Century of Struggle: The Woman's Rights Movement in the United States (New York, 1959, rep. 1970), 23-24; Holt Carleton Marlow, "The Ideology of the Woman's Movement, 1750-1860" (unpub. diss., 1966), ch. 8; Keith E. Melder, "Mask of Oppression: The Female Seminary Movement in the United States," paper delivered at the April 1972 meeting of the Organization of American Historians.

¹¹Letters and Papers, vol. V, 5, 31, 34, 39, 202-04, 262. When James Boswell told Samuel Johnson that he had heard a woman preach at a Quaker meeting, Johnson reportedly replied on July 31, 1763: "Sir, a woman's preaching is like a dog's walking on his hinder legs. It is not done well; but you are surprised to find it done at all."

¹²Letters and Papers, vol. V, 29-31, 37, 39, 139, 202, 214, 217; Earle, Dames and Wives, 87. It should be noted that in the published version of the Colden papers, none of her plant descriptions are included, but her 1756 "Cheese Book" detailing her production and sale of cheese is presented in full.

¹³The grievousness of the sin of idleness permeated colonial literature, especially among the Puritans and was considered particularly offensive when children, women and servants succumbed to it. See Cotton Mather, Ornaments for the Daughters of Zion, or the Character and Happiness of a Virtuous Woman (London, 1694), 98 and passim; Edith Abbott, Woman in Industry: A Study in American Economic History (New York, 1910), 34; Edmund S. Morgan, The Puritan Family (New York, 1966), 65, 66-67, 74, 147.

¹⁴The idea that there was an "Appropriate Sphere" of activity for women distinct from that of men abounded in religious statements and works of the colonial period and "for reasons not now clear, the divergence between male and female sex roles accelerated in the early 19th century," greatly aided by secular as well as religious speculation on the topic immediately following the American Revolution. See Marlow, "Ideology of the Woman's Movement," ch. 7; Keith Eugene Melder, "The Beginnings of the Women's Rights Movement in the United States, 1800-1840" (Diss. Yale 1963), ch. I; and their respective bibliographies. However, even with the sex-determined restrictions which existed for women scientists, it should be remembered in other areas of activity, especially those involving economic functions, "conditions for women . . . were more favorable in colonial days than they became in the early nineteenth century" for there are "evidences of independence and initiative on the part of (colonial) women, and . . . acceptance of these qualities by (that) society" which no longer exist today. See Dexter, vii, xxi-xxii.

¹⁵Letters and Papers, vol. V, 185-86; Spruill, 268-69; Leonard, 208-21; Bernal, Science in History, vol. II (Cambridge, 1971), 599-617.

¹⁶Earle, 87; Bernal, vol. II, 516, 636-38.

¹⁷A framed copy of this 1715 patent granted to Thomas Masters for his wife's invention can be found in the Franklin Institute, Philadelphia.

*Pursuant to his M^{sties} Grant for the using trying and preparing the Indian Grain fitt for transportation & Which was never before done these are Example of part of the Engines I [obscure] on my protection With the Witnesse my hand and Seale

SIBILLA MASTERS

In tertio die Novembris Anno MD^{ccxvi}Georgijanno tertio.

¹⁸Samuel H. Needles, "The Governor's Mill, and the Globe Mills, Philadelphia," Pennsylvania Magazine of History and Biography, VIII (1884), 285-86. Thomas Masters received another patent for a second invention of his wife[s] in 1716. This one was for the manufacture of straw hats and bonnets.

¹⁹Bernal, vol. II, 657-58.

²⁰Earle, Dames and Wives, 85-86; Ravenel, Charleston, 124-25, 390; Loutrel W. Briggs, Charleston Gardens (Columbia, S.C., 1951), 12; Mary B. Prior, "Letters of Martha Logan, 1760-1763," South Carolina Historical and Genealogical Magazine, LIX (1958), 38-46; Elizabeth H. Jervey, "Martha Laurens Ramsay; Eulogy," South Carolina Historical and Genealogical Magazine, XXXVI (1935), 136-37.

²¹Carl Holliday, Woman's Life in Colonial Days (New York, 1960), 78, 81, 295-96; Ravenel, Eliza Lucas Pinckney, 7-9, 14, 31-38, 102-07; Mary R. Beard, ed., America Through Women's Eyes (New York, 1933), 33-41.

²²Spruill, 267-75; Leonard, 219; Ehrenreich and English, 18-32; Charles E. Ziegler, "The Elimination of the Midwife," Journal of the American Medical Association, LX (1913), 33-36; Frances E. Kobrin, "The American Midwife Controversy: A Crisis of Professionalization," Bulletin of the History of Medicine, (July-August, 1966), 350.

²³Following the American Revolution there was a general decline in the relative status and position of all women, not simply those who were medical practitioners. It was caused by continued educational deficiencies often under the guise of educational reform such as the female seminaries; codification of American law which resulted in curtailing certain economic and legal privileges women had enjoyed in the colonial period; introduction of

the factory system in which women and children were encouraged to work; a complete ban on political activities for women which lasted until the middle of the nineteenth century; and an industrial prosperity which encouraged self-made middle-class men to obtain more and more luxuries, including an idle, ornamental wife isolated within the structure of the nuclear family. See Benson, 102-03, 157, 160; Dexter, 190-93; C. Willett Cunningham, Feminine Attitudes in the Nineteenth Century (New York, 1936), 201-35; Melder, "Mask of Oppression."

²⁴For confirmation of the need for a conceptual framework to deal with the subject of women in American history, see Gerda Lerner, "New Approaches to the Study of Women in American History," Journal of Social History, III (1969), 53-62; Banner, "On Writing Women's History," 347-58; Berenice A. Carroll, "Women As A Force in History," paper delivered on April 16, 1971, at the annual convention of the Organization of American Historians; Godon et al., "Historical Phallicies," passim; Ruth Rosen, "Sexism in History or, Writing Women's History is a Tricky Business," Journal of Marriage and the Family, XXXIII (1971); 541-44. For confirmation of the repetitious reliance upon the same sources and interpretations, one has only to consult the material cited in Eugenie Andruss Leonard, et al., The American Woman in Colonial and Revolutionary Times, 1565-1800 (Philadelphia, 1962).

Women's Colleges

By the mid-1800s, education for upper-class males in America had become an institutionalized process, and several reform-minded women and men were beginning to agitate for improving the education of females. (For a general history of education for women, see Eleanor Flexner's Century of Struggle, chapters 2 and 8). Colleges for women had been established prior to the Civil War, but Vassar, Wellesley, and Smith, which opened in the decade following the war, were the first women's institutions to have high academic standards for admission. Vassar and Wellesley had preparatory departments in which students could make up any educational deficiencies before beginning their collegiate program; Smith from the beginning admitted only students who met the same entrance requirements, including higher mathematics and Greek, as those demanded by the best men's colleges.

Women's colleges had a less structured curriculum than men's colleges, which tended to adhere to the traditional classical mode of training. For this reason women's colleges adopted the teaching of scientific subjects and used laboratory instruction earlier than many men's colleges. Science education at the new women's colleges varied widely, as did their faculties' abilities in these fields. Vassar College was fortunate in having Maria Mitchell (1818-1889) on its faculty from the time the college opened its doors in 1865 to shortly before her death. She was already a distinguished astronomer by this time, which was advantageous because she served both as a role model to her students and as a counter-example to the prevalent notion that women were incapable of scientific thought because their brains weighed less than those of men!

MARIA MITCHELL: THE ADVANCEMENT OF WOMEN IN SCIENCE

by Sally Gregory Kohlstedt

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Astronomer Maria Mitchell (1818-1889) was well known among her contemporaries as the first woman to win an international medal, the first American woman elected to the American Academy of Arts and Sciences, the first woman member of the American Association for the Advancement of Science, and the first woman professor of astronomy. Her success served to highlight the paucity of recognized women scientists in the nineteenth century. The barriers to women in science were high, limiting even those on the periphery of the scientific community as illustrators, textbook writers, and herbarium owners. (1) Thomas Henry Huxley argued that

since women were ipso facto amateurs, they were not worthy of membership in learned societies. (2) Similarly, access to institutions offering high levels of scientific education was negligible until the end of the century. Other more subtle factors also prevented women from realizing their scientific skills. Most women, influenced by the sexual stereotypes of Victorian society, lacked sufficient confidence to reach their full potential. It is significant that Maria Mitchell, having to some extent surmounted these obstacles, chose to work directly with women rather than to defy male hegemony. Her decision appears to be based on a belief that women's self-definition contributed to their limited participation in science; she felt that women had a fundamental problem with private confidence as well as public encouragement. Her initiative regarding women in science offers an illuminating perspective on a period typically viewed as indifferent to women's rights. (3)

Mitchell became a symbol to her contemporaries, men and women alike, of the contributions women were able to make in science. Her discovery of a comet in 1847, and her calculation of its exact position at the time of discovery, won her a gold medal from the King of Denmark and membership in the American Academy of Arts and Science, "in spite of being a woman. (4) One result of these honors was Lucretia Mott's citation of Mitchell's achievement at the Seneca Falls Women's Rights Convention in 1848 as evidence of women's capability in all occupations. (5) Aged thirty at the time, Mitchell was an independent, hard-working librarian and astronomical observer on Nantucket Island, far removed from reform activism. (6) Public recognition, however, gave her opportunity to travel, not only to meetings of the American Association for the Advancement of Science, but also to Europe where in 1857 she met leading astronomers, including Caroline Herschel and Mary Somerville, and visited their observatories. (7) Shortly after her return a group of women led by Elizabeth Peabody raised funds for an Alvan Clark equatorial telescope to enable Mitchell to make more precise observations. (8) Appointment as field researcher and computer for the Nautical Almanac--not coincidentally she was asked to compute the position of the planet Venus--permitted her more time for astronomical study. Mitchell was perhaps the only American woman to have self-supporting scientific employment and international recognition in the 1850's.

She knew from experience that self-help was slow, tedious, and sometimes unrewarding. Through her father's influence she won astronomical friends at Harvard. As the first woman in a number of male societies, she never felt the force of absolute exclusion; but she learned that nominal membership conferred little sense of collegiality. At some sessions of the American Association for the Advancement of Science, she attracted more curiosity than professional interaction. (9) Such experience helps explain why she grew less attentive to the problems of discrimination than to encouraging the aspirations of young women.

Mitchell had two stated incentives for involving women in science. Like many contemporaries she viewed science as a way of thinking. Concerned about the "half-educated, loose and inaccurate ways" in which women seemed to think and act, she hoped that scientific education would

provide a rational way of problem solving. She also believed that science offered a unique intellectual challenge and could help women escape the narrowness of their lives. Her crusade to involve women in science thus embodied both a concern for social progress and a commitment to expand opportunities for individual women.

Her decision to work with women is not surprising. As Carroll Smith-Rosenberg and others have noted, the period was one during which spheres of public and private activity were largely identified by sex. (10) Coming from Nantucket where Quaker women were often responsible for civic and economic matters while their husbands were at sea, Mitchell appreciated the effectiveness of women's networks. Although aspiring professionals might ultimately challenge the boundary of the sphere traditionally assigned to women, they had been shaped by its requirements and used its sources of strength. Mitchell's opportunity came with an offer to teach at Vassar. She agreed with a trustee who felt students should be "fed from living springs rather than a reservoir," and should be a fount of knowledge and inspiration. (11)

Her course in astronomy proved challenging. No student was admitted who had not passed an examination in mathematics. Juniors were given extensive reading assignments, but each Monday Mitchell counterbalanced these with lectures on the history and meaning of science reputed to be "racy and interesting" and which had a "more literary than mathematical" connection with astronomy. (12) Mitchell challenged but put no distance between herself and her students (their respect was evident in the fact that she was the only faculty member not dubbed with a nickname); she opened her advanced class with the comment, "We are women studying together." (13) She took students to New York City and into the countryside to make astronomical observations. She also invited prominent friends, including Julia Ward Howe and Mary Livermore, to the campus. Although Livermore avoided the controversial topic of suffrage, the group discussion led to agreement that "every girl should have some aim and some means with which to support herself in time of need." (14)

Vassar was exhilarating, but there were also frustrations. Perhaps jealousy prompted one objection that relationships might be too close between the teacher and certain astronomy students. (15) Although such rumors gained little attention, their existence indicates how easily suspicions arose about an unmarried woman scholar. Pressures were various, but student anxiety was a source of concern to Mitchell. Julia Pease, for example, whose early college letters frequently mentioned Mitchell's classes, was discouraged by disapproving parents from selecting advanced courses in astronomy. (16) Even those who remained in the class faced the question of careers after graduation. One student wrote:

I have Miss Mitchell and all these grand instruments, and nobody makes fun of it all here. But when I get home no one there will take any interest in astronomy. I shall have no telescope at first, and there will be no one there to help me on. Do you think I shall be brave enough then to hold on tight to what I have begun? When I think of it so, I get discouraged. (17)

The problems of her students reinforced Mitchell's conviction that offering women an opportunity to study was not sufficient to insure their participation in science. Within a year after coming to Vassar she resolved, "in case of my outliving father and being in good health, to give my effort to the intellectual culture of women without regard to salary." (18) Women scientists needed to be recruited in greater numbers and society needed to be made ready for them.

Englishwoman Frances Bacon Cobbe observed that "as women grew older, if they had led independent lives, they tended to be women's rights women." (19) This was certainly true for Mitchell. Maturity brought confidence in her own powers of intellectual and leadership abilities. More significant, she became committed to women's rights. Once settled at Vassar, she went often to Boston, visiting friends and attending lectures on a wide range of social issues. (20) Her travel journals for 1857 and 1873 reveal a remarkable change in her concerns. On her second trip to Europe in 1873, she again visited scientific friends and astronomical observatories; but now her primary concern was women's higher education. (21) She sought out persons actively working for women's education, and frequently discussed women's issues with fellow passengers. (22) On her return, Mitchell for the first time joined a national women's movement. Vassar had shown the need to raise the aspirations of women, and she was ready to contribute her energy to a women's organization whose initial goals seemed broadly defined to include professional advancement. She had chosen not to deal directly with the male scientific community but to work with women. Her strategy was positive: to encourage public readiness and to prepare the coming generation psychologically and practically for acceptance as professional peers. It was to these ends that she directed her efforts in the Association for the Advancement of Women.

For a number of years there had been discussion about forming some umbrella organization to cover women's club activities on a larger than local scale. The successful initiative came from Sorosis, a group in New York City. In 1869 several Boston women had visited the newly founded society to discuss a women's "Parliament" without results. (23) Four years later, however, Sorosis decided to issue a "Messenger" to leading women involved in educational reform and in established women's clubs. Their intention was to unite women working in isolation in order to undertake a "careful consideration (of) the more important questions that affect our women's lives." (24) Maria Mitchell was among the persons signing a "call" for a meeting in September, 1873. (25) Specifically invited were women preachers, teachers, professors, artists, lawyers, business women, editors, authors, and "practical philanthropists." (26)

The Association won approval from leading mid-century reformers. A wide range of women responded to the theme of education: Elizabeth Peabody discussed coeducation at Boston University; Catherine Beecher stressed the need for endowments of women's colleges; and Elizabeth Cady Stanton offered thoughts on coeducation. Attendance by several hundred women revealed a measure of broad interest. Some papers were published, the participants were enthusiastic, and Sorosis itself gained more local

members. (27) A mid-year conference of officers decided to hold the second annual meeting in Chicago, thus setting a precedent for peripatetic sessions. The western meeting was intended to attract new affiliates and serve as a stimulus to women in other parts of the country; subsequent meetings were held in Syracuse, Philadelphia (in the year of the Centennial celebration), Cleveland and Providence.

Public opinion had changed only slowly about acceptable leisure-time activities for women outside the home. Local groups organized for religious or charitable work were approved because they seemed an extension of women's domestic and moral concerns. Involvement in mixed reform groups and women's rights associations had come under steady public criticism. This opposition, however, began to erode when prominent community leaders spoke out. The new Association, with its highly visible leadership, found the press sympathetic to "learned ladies," and several newspapers went out of their way to explain that these women were practical and hardworking--"not mere talkers" or radicals. Having anticipated criticism, one AAW member recorded her satisfaction that the "croakers and foggies handsomely acknowledged that the deliberations were hightoned." (28)

The formation of the AAW gave women with Mitchell's vision a valuable public forum from which to promote their ideas. The binding commitment was to education, formal and informal. In 1874 Mitchell's address on "The Higher Education of Women" traced the initiative of Emily Davies and others who had founded Girton College at Cambridge, England. While Mitchell deplored the unwillingness of the University to grant degrees to the graduates, she approved its intention to establish courses for women equal to the best male education. But where to start? Mitchell was practical and concluded with a plea to every women's club to investigate its local situation and to work for the education of women by campaigning for positions on school boards and by supporting women's colleges. (29)

Mitchell had substantial hopes for the AAW as a vehicle for reaching a wide audience of women. She recruited new members and solicited papers from friends and former pupils. (30) With Mitchell on the program committee, it is not surprising that several papers dealt with women and science. One was by a former student of Mitchell, whose attention had turned from astronomy to applied chemistry. Ellen Swallow (later Richards) asked, "What Practical Science is Open to Women?" Her paper pointed toward her own initiative on the subject of nutrition. Another read a paper on "The Value of Natural Science for Women." Probably her own address, her efforts to build an effective program, and her prestige led to Mitchell's election as second president of the AAW for 1875 and her reelection in 1876.

As president, Mitchell planned the annual meeting and organized the AAW for more comprehensive action. (31) Surprised by the administrative effort required, and reluctant at first to address large crowds, she proved to be a successful presiding officer. (32) Her presidential address was a review of the Association's origins and a series of suggestions for the future. While she praised the enthusiasm and dedication shown by women

at the first two Congresses, she urged that it was essential to create greater stability and to develop new programs. Her recommendations reflected her own approach to life in emphasizing that permanent change in the condition of women could come only by "combined, continuous, and systematic action." Mitchell recognized the effect that the Women's Congresses had had simply by virtue of annual meetings which brought together dedicated women from around the country, and awakened local women to "definite views, higher ambitions or braver efforts." Nonetheless she wanted to produce more concrete results, calling for statistics on women in education, industry, and such special problems as prison, poverty, or chronic ill health. Accurate statistics would, she argued, enable activists to indicate the number of girls who suffered from "aimless lives" as well as those who had studied too hard (clearly a return to Edward Clarke), the number of working women at all levels of employment, and the success of asylums for the poor and criminal. Statistics, too, could be a "formidable opponent to the flourish of rhetoric" bandied about with regard to women's presumed characteristics. Her presentation was tart, eloquent, and explicit when she demanded "fairness for the individual woman."

Mitchell's special concern remained with aspiring professionals and she tried to make the audience--composed primarily of volunteers--understand the problems confronted by aspirants. On the one hand, she noted, society underestimated women and thus women rated themselves too low. At the same time, "with our ideas of women, we demand too much of a woman. We are generous toward the species but severe upon the specimen." She cited the example of the president of a coeducational school who had indicated that he would hire a woman professor if he could find the equal of Mary Somerville. Mitchell argued that the demand was "enormous," and that if similarly each male appointment were the equal of Pierre de LaPlace, then college chairs "must be vacant." While applauding her contemporary pioneers, she expressed concern with the single-mindedness of the most dedicated and suggested, "there is something almost painful in the seriousness of the best girl graduates from our colleges." (33) Witty and concrete, Mitchell was quoted at length in newspapers which commended her for her "sturdy common sense, her scientific knowledge, her ready talk, her abundant good nature, her prompt application of rules, and her steady presence." (34)

At the Centennial celebration in Philadelphia, Mitchell redirected her attention toward the "Need of Women in Science." Earlier she had argued that women need the discipline of effective science teaching to improve their rational facilities. The AAW address, written in response to a debate among Thomas Wentworth Higginson, Mary Livermore, and Louis Agassiz on the capability of women at a Social Science meeting in Boston, approached the situation from a different perspective. (35) She argued that not only could women use scientific training, but that science also could benefit from women. Childhood training developed in girls certain skills and characteristics which made women meticulous observers of natural phenomena. Moreover, she pointed out, the increasing need for science education in the primary schools could only be solved by better preparation of the women who taught on this level. While Mitchell preferred to deemphasize

sex differences, she was pragmatic enough to mention women's socialized skills as an encouragement to women to engage in scientific studies.

Anticipating the query why so few women had contributed to science in the past, she gave an exaggerated comparison to make her point. Tycho Brahe, the Danish astronomer, had received a royal grant of an entire island, instruments, and assistants in order to pursue his research; this was the "ideal" intellectual life. In contrast, Mitchell cited excerpts from astronomer Caroline Herschel's diary; at fourteen Herschel attended church and school but at home was so busy at the scullery that she could rarely join in family discussion; at twenty she was knitting socks for her famous astronomer brother; and only at thirty-seven years did she gain a 30-pound annual income as his assistant which she recorded gratefully was "the first money I ever in all my life thought myself to be at liberty to spend to my own liking." Mitchell's conclusion, like that of Virginia Woolf a half-century later, was simple:

The laws of nature are not discovered by accidents; theories do not come by chance, even to the greatest minds; they are not born of the hurry and worry of daily toil; they are diligently sought, they are patiently waited for, they are received with cautious reserve, they are accepted with reverence and awe. And until able women have given their lives to investigation, it is idle to discuss their capacity for original work. (36)

Mitchell was all too aware of the cost of multiple responsibilities. Two years as president of the AAW, with details of administration and distracting controversies over personality and papers, were enough. (37) Skipping the Cleveland meeting in 1877 to avoid presiding in Howe's absence, she returned the following year to read a paper on the solar eclipse observed by her Vassar students. Public education, however, was only a first step for Mitchell. (38)

In order to support women with talent and provide an incentive for advanced work, Mitchell suggested that the Association sponsor a scientific society for women. Although a growing number of "ordinary" scientific societies were open to women, she pointed out that "there is really little room for papers by women and we have so long been accustomed to listen in silence, and not to speak, to receive views and not to advance them" that few women were ready to speak to mixed audiences as "equally unmixd as they are in science." (39) Her women's scientific society would be, in effect, a board of experienced researchers who could encourage younger women in laboratories and museums to present the results of their work. Mitchell envisioned a far-reaching society which would eventually establish courses, found schools, and develop local organizations. Women, once attracted to science, needed steady encouragement to offset the negative public response. Mitchell wanted to create an environment in which aspiring young women found concrete advice, encouragement, and the ways and means to pursue research. Dr. Mary Putnam Jacobi, already committed to professional specialization, responded skeptically:

I confess I do not see where you are to find enough women knowing enough about science to write about it, to form a committee,--and when you have,--who will be the audience to listen to them? or how would Prof. Mitchell and Mrs. Treat (a botanist) meet on common ground?--As for myself, I cannot more than any other practicing physician, claim to be a "scientist." (40)

Mitchell very shortly was forced to agree that there was not yet the critical number of women in the sciences required to form a workable organization.

No society was ever formed, but in 1876 the AAW created several committees, including one on science. Until her death in 1889 Mitchell served as its head. The Committee's purpose was to stimulate interest in science by reports "showing the fitness of women students to pursue investigations in different fields and their gradual assumption of responsibilities as professors and teachers." (41) Mitchell solicited information on the status and opportunities for women in science and medicine through an open-ended questionnaire. She asked:

1. Are there women in your community engaged in scientific research, or in original investigation?
2. Are there women who make a specialty of any branch of physical science, chemistry, or natural history, as teachers or as writers?
3. Are there women physicians, and have they a successful practice?
4. Are there scientific schools open to women?
5. Are there women who are giving pecuniary aid to such scientific schools? (42)

In 1876 Mitchell sent the questionnaire randomly to friends, former students, and institutions in various parts of the country asking that the circulars be returned even if every answer was negative. The number mailed is unrecorded, but by the time Mitchell made her report, she had sixty-five replies and fourteen arrived later. (43)

There were many negative responses and the net results confirmed Jacobi's prediction regarding the dearth of women engaging in scientific research. Mitchell tabulated the results, admitting that the numbers so casually acquired were useful only for general observations: 7 questionnaires were totally affirmative, 47 were partially affirmative, and 19 were totally negative. (44) The questions hinted at some relationship between scientific study and medical practice, but the two proved to be basically unrelated. In Albany, for example, the Dana Natural History Society (an amateur group of educated women) was reported as "large and reasonably prosperous," while the four women physicians in town had limited practices because, "Albany is desperately conservative." (45) Even more commonly there would be no reported activity in science but a number of successful physicians. The early nineteenth-century nexus of physician and scientist

was nearly over, with a few notable exceptions like the enterprising Sarah Dolley of Rochester. (46)

It proved easier to deal with the question of physicians, who formed a distinct professional group, than to identify women scientists. Who should be included? The scientists were themselves in the process of distinguishing amateurs from professionals and provided no clear standards. Respondents showed understandable confusion and their answers varied from an uninspired "yes" or "no" to discussion of the interests of specific local women. Question one, with the requirement of "original investigation" elicited only seventeen positive replies out of seventy-nine; twenty-nine persons were identified in answer to the second question. Altogether fifty-one women were named, most of whom were local school-teachers and members of natural history societies. The incomplete list, lacking such names as Almira Phelps, who had written textbooks in natural science; Emily Gregory, then studying at Cornell; and astronomy students like Mary Whitney, a Vassar alumna who eventually became Mitchell's successor there. Because Mitchell had relied on her own network of students and friends, there was a distinct regional bias; only Kentucky and Texas represented the South.

The questionnaire left no doubt that women were capable of scientific study. Women teachers in elementary and high schools, normal schools, and women's colleges taught every subject from natural history to chemistry and astronomy. Often, however, there was a note of apology. Emma Morrill, who belonged to an active botanical society in Wilmington, Delaware, wrote defensively, "Women are not idle, they are good workers, many of them efficient and capable, but their work is of a kind that cannot be put down in a schedule." (47) Women teachers had often "stumbled into the subject" of science and many, like young Mary Reed of Philadelphia, were assigned several areas with little opportunity to develop any special expertise. During the summer, however, Reed conscientiously attended summer school at Cornell in an effort to learn more about the subjects she taught. (48) The circulars also indicated efforts to secure opportunities for women seeking careers in science. Ellen Swallow Richards, who had become the first woman student of chemistry at the Massachusetts Institute of Technology, for example, wrote her former teacher about a proposal for women to study applied chemistry at the Institute. A joint committee of the Institute and the Boston Women's Educational Association had approved, and efforts by the latter were under way to raise financial support. Pleased with recent progress, Richards wrote optimistically, if naively, "There is no longer any doubt as to the ability of women to carry out any work of this kind." (49)

Indeed the list of women from the Boston area was impressive with twelve scientists and ten physicians. (50) Moreover, local women were gaining recognition. At the Boston Society of Natural History, where women were only "patrons," the question of membership had come under discussion. Lucretia Crocker, a former science teacher and member of the Boston School Committee responsible for developing a Saturday program for teachers at the Society's Museum, reported the sequence of events. Once women began studying science at the Museum, the "inevitable" had

happened: "At the close of a course of lectures or lessons by a Harvard Assistant Professor of Botany it was proposed to form a Botanical Section, when the dilemma arose--How can women join a section if they are not eligible as members of the Society?" (51) In less than a year several women had gained at least partial recognition as members in the new category of Associate Member of the Society. (52)

Mitchell found such news fascinating although she never made use of the names she gathered to form a scientific club. The data became a basis for her annual reports and in 1880 she circulated the same questionnaire but identified Henrietta Walcott and Antoinette Blackwell as new committee members. (53) This survey was apparently smaller and more select; Mitchell's manuscripts reveal only nineteen replies. Her report for 1881 stressed the progress made in five years. The best results related to women physicians, whose numbers appeared to be rapidly increasing. (54) Positive reports from Lucretia Crocker in Boston, and Grace Anna Lewis in Philadelphia, noted that if in 1876 a scientific institution had been partly open to women, by 1881 it was wholly so. (55) Yet she paused in her report to ask, "At what time did scientific associations close to women?" Princess Catherine of Russia had been chosen a member of the American Philosophical Society in 1789, and the American Academy of Arts and Sciences in Boston had elected fifteen members in 1780 without apparent regard to sex. (56) Yet both organizations had since closed their doors to women--with the exception of Mitchell's own election to the Academy in 1848 (the last woman until the twentieth century). Mitchell's efforts led to a more sophisticated understanding of history and the realization that progress could not be assumed.

Thus success appeared to her to be dependent on women working cooperatively to advance the situation of women. On the negative side she cited the paltry response to question five concerning women's financial support of other women and gave explicit credit to five persons, including Elizabeth Thompson, who contributed to the cause of women in science. (57) More positively she pointed to the research of faculty at women's colleges like Wellesley, Mount Holyoke, and Vassar. (58) She stressed the expansion and vitality of women's groups in Syracuse, Chicago, and other cities where the AAW had held annual meetings. Privately she subscribed to a "great woman" theory, recording in her diary with satisfaction, "Given one wide awake woman in a town . . . you can rouse the whole." (59)

Astronomer Mitchell was reluctantly forced by the questionnaire results to conclude that "those sciences which touch life and health" were most attractive to women. The defection of Mitchell's promising student, Ellen Swallow, to applied chemistry (analysis of purity in foods and water) underscored her observation that the "abstract sciences" (mathematics, astronomy, and physics) attracted only a small proportion of women entering science. Social consciousness helped explain the emphasis--a significant challenge to women's expanding rights came from arguments regarding health. (60) Opportunity was the other side of incentive. The life sciences led to medical schools which were admitting more women. (61) Although most medical graduates opened private practice, several respondents reported women working as superintendents in state and charitable

institutions, medical directors in gymnasiums, members of city or county boards of physicians, medical missionaries, and delegates to medical conventions. (62) Such facts seemed to indicate that women were being integrated into regular medical circles and in the 1880's most women expected the pattern to continue.

The emphasis of her reports was not on those who enjoyed science avocationally, but those who took it up as a "serious and life-long occupation." (63) She described the work of women who presented their results at exhibitions, who published in Popular Science Monthly, who attended and read papers at professional meetings, and who held salaried positions in science. Employment was a problem and Mitchell's reports began to document a variety of possibilities, mentioning women as naturalists and assayists with mining companies and government surveys, as computers at observatories, as pharmacists, as assistants at the Smithsonian Institution and at the Museum of Comparative Zoology in Cambridge, and as teachers on every level. Gains in science were less dramatic than those in medicine and, cautioning against her own impatience, Mitchell insisted, "measured by a year's interval, the scientific study of women shows little change. If we take half a century, the growth is astonishing." (64)

Mitchell allowed the science reports to dwindle in content throughout the 1880's. (65) A partial explanation may have been fatigue; Mitchell was in her late sixties. In addition, she found the Association less personally rewarding and professionally significant than she had hoped. (66) The character of annual meetings shifted as the leadership of the AAW became dominated by philanthropic leaders with leisure time. The Committee on Science, as conducted by Mitchell, seemed an anomaly representing professional aspirations in the midst of the voluntarism and moral philanthropy advocated by Julia Ward Howe, who served last and longest as AAW president from 1880 to 1900. In retrospect the AAW met its stated purpose of disseminating information and inspiring local women at annual meeting places into literary or reform-minded groups. (67) Yet, ironically, the Association's effort to be a stimulus rather than coordinator of local clubs meant that some of the very groups it helped initiate eventually joined the historically more visible General Federation of Women's Clubs, founded in 1889.

The Association initially had stressed working activists. The Sorosis "Messenger" of 1873 expressed the hope of uniting foremost writers and thinkers, women working in either traditional or nontraditional areas, and practical philanthropists. (68) Perhaps the needs of these groups were too diverse to be united, although representatives of each undoubtedly joined the AAW in order to have a national base. By 1882, the AAW had sufficient support for a regular publication, and the group was beginning to lose its membership to other organizations. Mitchell's own former students, including Christine Ladd-Franklin, Ellen Richards, and Martha Talbot--women who might have been expected to play a larger role in the AAW--instead helped to organize women college graduates into the Association of Collegiate Alumnae, directed particularly toward women who were themselves career-minded or ready to support others who were. (69)

Since Mitchell had expressed some dismay over the quality of AAW leadership as early as 1875, it is not surprising that she gave full support to the new collegiate association. (70)

Mitchell's own prodigious activity and vigor of the early years of the AAW belie any assertion that feminism was quiescent in the 1870's. While the General Federation and the reunited suffrage movement have made the 1890's appear to be a take-off period for women's activism, the 1870's and 1880's were crucial for raising the consciousness of numerous women and laying the groundwork for later coordinated activism. The AAW discussed many topics--the advancement of women's higher education, the enlargement of women's interests, the strengthening of responsibility within the home, health, physical development, and sensible attire--thus introducing significant topics to groups of women not likely to subscribe to a feminist journal or to have much contact with women in their geographical regions. The leaders of the AAW were, as newspapers often observed, eminently practical. The AAW's low profile may have helped obscure the group's considerable influence, as viewed by historians as well as many contemporaries.

The influence was indirect. Mitchell's "statistical investigations" were never applied to problems. The Committee on Science, like the Association, simply supplied information to arouse women's awareness but left to other groups specific programs of action. In the case of the science society, Mitchell's hopes were premature; the critical number of women involved in research did not yet exist. Without such groups, women had little leverage within professional circles. In 1879, for example, Mitchell recorded, "I wanted to nominate some women in some of the comm. of the Association (for the Advancement of Science) but my friends assured me that I should do more harm than good." (71) The slow, persisting work of the Science Committee and Mitchell could only lay groundwork for later activism by encouraging segmented efforts of isolated women to enter scientific careers and to continue their private study. (72)

The foundation laid by Mitchell and others had been hidden because it was built outside the scientific community and in conjunction with an organization which flourished only briefly. Mitchell had helped invigorate the early years of the Association. She had lent her name and shared her vision from the presidential podium and then stepped down to head the Committee on Science, whose enterprising questionnaire and reports became a model among the AAW committees. Her own work was presented in sufficiently popular form to win attention from local audiences which frequently numbered over 100 persons and she solicited papers from others in science and medicine to demonstrate the quality of women's research. Her public posture was deliberate. On the one hand, she spoke out for women's education and represented a fulfilled single career woman. On the other hand, she avoided public statements about suffrage and avoided opinions that might offend. (73) It is evident that her own astronomical work suffered from her commitment to help other women overcome barriers.

The AAW questionnaires indicated a substantial number of women interested in science and hinted at their rising aspirations. Although

other women, including friends like Grace Anna Lewis, Antoinette Brown Blackwell, and Lucretia Crocker were influential in this movement, it is difficult to exaggerate Mitchell's key role in the 1870's. Influence cannot be measured quantitatively, but after the 1876 meeting Phebe Hanaford placed Mitchell at the "head of the list of scientific women in America." (74) Mitchell held a vision and pursued strategies which coincided with changing but cautious views of women's clubs: she sought and utilized the psychological and practical reinforcement found in a female world. Mitchell, in her later years at least, found homo-social society congenial and a more useful vehicle for change than any direct challenge to male bastions. She believed that the integration of women into professional circles could come only after women had gained, within a supportive environment, confidence and expertise. Her strategy was to create an information center and visibility for women's individual successes. The remarkable increase of women in science by the turn of the century suggests that her own students and younger friends implemented her long-term goals of women's collective action and professional participation.

Footnotes

The author would like to thank colleagues and friends who commented on an earlier version of this paper, especially Janet Bogdan, Margaret Rossiter, and Robert Post.

¹This activity is discussed in my essay, "Working in from the Periphery: Women in Science in America, 1830-1880," to be published in Signs. For the late nineteenth century see Margaret Rossiter, "Women Scientists in America before 1920," American Scientist, LXII, 312-323 (May-June, 1974). The few books which deal with women and science tend to recount heroines' efforts without much attention to the mutual support networks; among the best are Edna Yost, American Women of Science (Philadelphia, 1943); Madeleine B. Stern, We the Women (New York, 1962); and Phebe Kendall, Maria Mitchell, Her Life, Letters and Journals (Boston, 1896).

²Quoted in Joan N. Burstyn, "Sex and Education: The Medical Case Against Higher Education for Women in England, 1870-1900," American Philosophical Society, Proceedings, CXVII, 79-89 (April 10, 1973). Contemporary John Zahm (under pseud. H. J. Mozans) in Women in Science (New York, 1913) wrote (p. 105), "Perhaps the greatest change is that woman now does thoroughly what before she did only as an amateur." Standard accounts of the professionalization process tend to ignore the subgroups excluded for reasons other than ability: George Daniels, "The Process of Professionalization in American Science: The Emergent Period, 1820-1860," Isis, LVIII, 151-166 (Summer, 1967); and Daniel H. Calhoun, The American Civil Engineer: Origins and Conflict (Cambridge, 1960).

³Mitchell's biographers tend to deemphasize her women's rights activism, especially her sister Phebe Kendall. An exception is Dorothy J. Keller, "Maria Mitchell, An Early Woman Academician" (Ph.D. dissertation, University of Rochester, 1974). Most recent analyses of women's activism

have concentrated on the antebellum period or on the 1890's with the emergence of the General Federation of Women's Clubs, the reunited National American Women's Suffrage Association, and the rise of trade unionism and socialist feminism. The tendency is to see the 1870's and 1880's as a premodern interlude.

⁴The Academy's Report for 1848 is quoted in Keller, "Maria Mitchell," 3.

⁵Eleanor Flexner, Century of Struggle: The Woman's Rights Movement in the United States (Cambridge, 1959), 71-77.

⁶Her diary has no comments about the role and status of women until about 1855. Most of the Mitchell manuscripts are at the Maria Mitchell Science Library on Nantucket Island (hereafter MMM) and available on microfilm at the American Philosophical Society in Philadelphia; some correspondence is preserved in the Vassar College Archives (VCA).

⁷The trip and the meetings with Herschel and Somerville seemed to redirect her attention toward the role of women in science. Throughout her career Mitchell would use the two women as examples of women's underutilized potential. See her "Reminiscences of the Herschels," Century Magazine, XXXVIII, 903-909 (Oct., 1889).

⁸Helen Wright, Sweeper in the Sky: The Life of Maria Mitchell, First Woman Astronomer in America (New York, 1949), 104 and 120. A late notebook entry (undated, item 35, MMM) by Mitchell acknowledges three crucial aids to her success: the woman teacher who taught her to love learning, the man who loaned her \$100, and the women who gave her the telescope.

⁹Wright, 69.

¹⁰"The Female World of Love and Ritual: Relations between Women in Nineteenth-Century America," Signs, I, 1-30 (Autumn, 1975).

¹¹Mitchell to Rufus Babcock, March 18, 1964, VCA.

¹²"Maria Mitchell at Vassar," unidentified clipping, item 35, MMM. Students considered Mitchell "sternly just yet marvelously kind." See comments in Catherine Weed Barnes and Ella Dietz Clymer, "Report to the Delegates from Sorosis," in AAW, Report of the Fifteenth Women's Congress held in New York City, October, 1887 (Fall River, Mass., 1890), 26; and news clippings from Toronto Mail (Oct. 16, 1890); and undated Toronto Globe in the Julia Ward Howe Scrapbooks, II, 5, 52, Schlesinger Library on the History of Women in America, Radcliffe College, Cambridge.

¹³Kendall, Maria Mitchell, 31.

¹⁴Julia M. Pease to Momma, May 2, 1875, Student Letter File, VCA. The series of letters from Jan., 1872, to May, 1875, suggest the influence a faculty member might have on the life of her students.

¹⁵Martha LeB. Goddard to [Sarah J.] Spaulding, Boston, Sept. 24, 1875, Mitchell Mss, VCA. Goddard was incensed at some "libel" by a "coarse, disloyal, treacherous" Vassar student who "told the secrets of her sex, and told them in so false a way that she lied--even if her facts were true." Goddard expressed none of the fears of homosexuality which emerged in Victorian America when she wrote, "almost every girl has for her first love, an older girl or some young woman, often her school-teacher, to whom she gives a pure and adoring affection, and to whom she offers gifts as she dares--Haven't you many a time saved your sweetest flowers, and done your finest handwork for some woman whom you loved?" This period may have been a turning point from the acceptance of the close woman-to-woman relationships discussed in Smith-Rosenberg's "Female World of Love and Ritual."

¹⁶Julia Pease to Carrie [Pease], Vassar, Jan. 17, 1875, Student Letter File, VCA; and John H. Raymond to Mitchell, Vassar, Monday (1875), Mitchell Mss, VCA.

¹⁷Gertrude Mead to Mary Whitney, Vassar, Sept. 29, 1870, Astronomical Observatory Notebooks, VCA. A letter to Miss Hopson dated only Nov. 25 indicates that Mead might be willing to sell her Clark telescope, thus indicating that she did drop her astronomical study. Mitchell, Mss, VCA. The number of unsuccessful aspirants to science careers can never be tabulated but evidence exists about thwarted hopes; see the letters from Alice Bache Gould to Elizabeth Agassiz, Agassiz Mss, Schlesinger Library, Radcliffe College.

¹⁸Miscellaneous Notes, Oct. 18, 1868, item 35, MMM.

¹⁹European Notes, item 49, MMM. Mitchell interviewed Cobbe and discussed the role of women, concluding, "The world would never know how much it had lost by Mrs. Somerville's not having been educated."

²⁰June 3, 1878, item 29, MMM. She attended meetings of the New England Women's Club; on one occasion she sat impatiently through a lecture by Harvard mathematician Benjamin Pierce who argued "that women were the educators of men and should be satisfied with that." Notebook, item 29, MMM.

²¹Journal, item 48, MMM. The journal for the second trip is filled with detailed notes of interviews with educators in Edinburgh, Cambridge, and London.

²²Wright, Maria Mitchell, 197-204.

²³These included Mrs. Horace Mann, Elizabeth Peabody, and Mrs. Charles Pierce. J.J. Croly, Sorosis: Its Origins and History (New York, 1886).

²⁴Papers [of the AAW]...1876 (Todd Brothers, 1877), 121-122.

²⁵ Mitchell was listed as an Honorary Member in Marguerite Dawson Winants, A Century of Sorosis, 1868-1968 (Uniondale, New York, 1968), 113.

²⁶ Papers...1876, 123. The planning committee estimated that 1,620 letters and notices had been mailed to women in the United States and Europe.

²⁷ Lita Barney Sayles, History and Results of the Past Ten Congresses of the Association for the Advancement of Women, published separately and as part of the Papers Read Before the Association for the Advancement of Women at Its Tenth Annual Congress (1882), 6-7. There is no study of the Association for the Advancement of Women, whose history has been eclipsed by the more long-lived, activist General Federation of Women's Clubs, which assumed some of the Association's earlier functions in the 1890's. The best accounts are contemporary, including Sayles; Julia Ward Howe's Reminiscences; Laura E. Richards and Maude Howe Elliott, Julia Ward Howe, 1819-1910, I (Boston, 1916); and Howe's Historical Account of the Association for the Advancement of Women, 1873-1893, World's Columbian Exposition, Chicago 1893 (Dedham, 1893); the latter has some inaccuracies repeated in subsequent accounts. The Association published reports from 1873 to 1893, although those to 1881 are only lists of papers and some summary of business items. Detailed reports of committees and state vice-presidents began in 1882. Selected papers, although numbered in sequence, 1873-1891, were apparently published from 1873 to 1876 and from 1881 to 1891. The most complete series are housed in the Vassar College Archives. There is apparently no manuscript record of the AAW although the Howe papers at the Schlesinger Library have considerable material relating to the late 1880's and 1890's. A series of letters from Howe to Emily Howland between 1888 and 1894 indicate Howe's unsuccessful efforts to find younger members, a more efficient governing system, and money for the organization; see Sophia Smith Collection, Northampton, Massachusetts.

²⁸ Sayles, History and Results, 9. Local AAW officers themselves printed disclaimers in local newspapers to allay fears of the potential audience: "It has been announced that women's right to suffrage, marriage, divorce, free love, and Bloomerism will not [be] considered in the deliberations of the Congress," Fayetteville Recorder, Oct. 14, 1875; and "It will be seen from the list of topics printed above, that the Congress will have nothing to do with the discussion of women's suffrage, or other topics connected with what is popularly termed the 'women's rights' movement," Syracuse Journal Sept. 18, 1875. Clippings file of the Onondaga Historical Society, Syracuse, New York.

²⁹ Papers and Letters Presented at the First Women's Congress of the Association for the Advancement of Women (New York, 1974), 89-98.

³⁰ Note to Miss Lapham, n.d., attached to a reprint of "Notes on the Satellites of Jupiter," Mitchell Mss, VCA; also see Mitchell to Antoinette Brown Blackwell, Jan. 19, 1878, Blackwell Family Mss, Schlesinger Library, Radcliffe College.

³¹"Address of the President," Papers...1875, 3. Commonplace Book, Nov. 2, 1873, item 50, MMM. Mitchell wrote, "I am amazed that they considered me a good presiding officer. I made my little speech boldly and fearlessly," and emerged from the meeting feeling "fifteen years younger." Howe, - Reminiscences, 387.

³²Confronted with an almost immediate controversy about whether to have an opening prayer, the pragmatic Mitchell compromised on the spot with a minute of silent prayer (over according to one reporter, "in half that time"). Woman's Journal, Oct. 23, 1875.

³³"Address of the President", Papers...1875, 3.

³⁴Syracuse Journal, Oct. 18, 1875.

³⁵Journal for 1873, item 48, MMM. Higginson gave an address on the higher education of women and Agassiz took exception to critical comments about Harvard, insisting that he made no distinction among his assistants. Mary Livermore asked why her daughter could not get a regular college education in Boston or Cambridge. Mitchell recorded the debate in detail and concluded optimistically, "It is clear that learning is to be made possible for women; that they shall accept it and value it as the duty for them, and that they shall use it wisely. For myself it seems to me my duty is to prepare a Lecture on 'Science for Women' and in the claim to their consideration which I think it has."

³⁶Papers...1876 (Washington, D.C., 1877), 9-10.

³⁷The meeting in Philadelphia was not pleasant. The assigned hall had problems and the Philadelphia women threatened to cancel their sponsorship if suffrage were mentioned. When Howe was nominated for President, the western representatives shouted the names of western women, including controversial temperance leader Frances Willard; ultimately Mitchell was accused of "carrying things with a high hand." Copy book entry, Nov. 15, 1876, item 50, MMM. Mitchell confessed, "I admire the courage of people who can work with all kinds of people, but I cannot do good work in inharmonious relations and must keep free of them." Mitchell to Alice Stone Blackwell, Jan. 19, 1878, Blackwell Family Mss, SLR.

³⁸Participants reported on summer school on Penikese Island under Agassiz and at Harvard's Botanic Garden in which women were given unprecedented opportunity to study. "Our Museums and Our Investigators," Papers...1876, 26-28.

³⁹"Lectures on Women," undated item 17, MMM.

⁴⁰To Antoinette Brown Blackwell, March 22 [1879], Blackwell Family Mss, Schlesinger Library, Radcliffe College.

⁴¹When too busy to work on the committee reports, Mitchell solicited papers on education and science. Mitchell to Blackwell, Jan. 19, 1878, Blackwell Family Mss, Schlesinger Library, Radcliffe College. Upon

leaving the presidency she resolved to "push the Scientific Dept. going a little. It is all very uphill work--mainly because women have so little money or control." Copybook, item 50, MMM.

⁴²Item 47, MMM.

⁴³Item 47, MMM. Although the circulars are missing, a total of seventy-nine responses are recorded in a copybook apparently complete and roughly organized by state.

⁴⁴Item 47, MMM. Mitchell's own tabulations are at the end of the copybook.

⁴⁵The Dana Society, composed largely of social leaders, concentrated on self-help and on local park projects; there seemed to be little interest in careers for women in science. The Society's manuscripts are deposited at the Albany Institute and Historical and Arts Society, Albany, New York.

⁴⁶Dolley apparently wrote to every woman about whom she had heard in upstate New York but none of them felt they were contributors to science. Within three years, however, Dolley again wrote to Mitchell, describing the newly founded Rochester Society of Natural History which was composed chiefly of women; item 46, MMM. Also see the Society's manuscripts and annual reports for 1880 and 1882 at the Rush Rhees Library, University of Rochester, Rochester, New York.

⁴⁷Delaware similarly reported an active study group but no women physicians.

⁴⁸Item 46, MMM. A note from J.J. Burleigh to Grace Anna Lewis pointed out that Reed had applied unsuccessfully to the summer school on Penikese Island; subsequently she enrolled at Cornell.

⁴⁹Item 46, MMM. Richards wrote a long note to her former teacher, explaining the situation in Boston.

⁵⁰Item 46, MMM. F. E. Foote also noted the variety of educational opportunities in the Boston area, including the special courses at Harvard, the scientific course at the Lowell Institute, the summer course in Salem, and the students at the Technical Institute.

⁵¹Item 46, MMM.

⁵²Sally Gregory Kohlstedt, "The Nineteenth-Century Amateur Tradition: The Case of the Boston Society of Natural History," in Science and its Public: The Changing Relationship, Gerald Holton and William A. Blarup, Editors (Holland, 1976), 185-186.

⁵³Annotations in her copybook for 1876 indicate the second mailing in 1880 was much smaller. Mitchell had only nineteen replies by the time of her report. A list dated Aug. 20, 1880, contains the names of twenty-one

women in various states (most of whom had sent replies in 1876) and five high schools; responses outside this group indicate that Mitchell must have sent additional letters and that not all inquiries were answered.

Walcott was a close friend of Howe and active in the AAW, sometimes making the report in Mitchell's absence. Blackwell, whose The Sexes Throughout Nature: An Inquiry into the Dogma of Woman's Inferiority to Man (New York, 1875) contained a "new scientific estimate of feminine nature," had relinquished her ministerial work in favor of scientific and philosophical study. Blackwell is a fascinating woman who deserves a full biography.

⁵⁴ Mitchell suggested that "when any town has a [woman] physician in 1876, it has several in 1881." Draft copy of Mitchell's report for 1881 is in item 24; the published reports began in 1882.

⁵⁵ Crocker had described the Associate status of members in the Boston Society, expressing her hope that women would soon be made regular members. Lewis submitted a list of women admitted to the Academy of Natural Sciences since 1876. Although there was a movement toward specialized societies in the 1870's, these women were of the generation which still found local acceptance important.

⁵⁶ Item 46, MMM. Endowment was a major theme in her address, The Collegiate Education of Girls, read in 1880 before the AAW and published separately in Boston in 1881.

⁵⁷ Item 21, MMM. On April 26, 1882, Mitchell recorded a visit from Thompson during which Thompson offered to pay Mitchell's expenses as a summer travel companion in Europe. Although Mitchell declined, she admired the cheerful, unconventional woman who was not "weak-minded." For a discussion of Thompson's scientific philanthropy, see Howard S. Miller, Dollars for Research: Science and Its Patrons in Nineteenth-Century America (Seattle, 1970), 127-129.

⁵⁸ Margaret Rossiter's book-length study, in process, on women in American science makes clear that the women's colleges were havens for serious students and productive women scholars.

⁵⁹ Notes and Diary, Aug. 20, 1881, item 24, MMM.

⁶⁰ The debate was taken up with vigor in Edward Youman's Popular Science Monthly in the 1870's. Also see Rosalind Rosenberg, "In Search of Woman's Nature, 1850-1920," Feminist Studies, III, 141-154 (Fall, 1975).

⁶¹ Diary for 1883, item 21, MMM. Mitchell quoted E.B. [sic] Stanton as stating there were "470 women physicians with university diplomas in the United States."

⁶² Report...1887, 34-35. For discussion of this optimism and subsequent disillusionment in the twentieth century see Mary Roth Walsh, Doctors Wanted: No Women Need Apply, Sexual Barriers in the Medical Profession, 1835-1975 (New Haven, 1977).

⁶³Report...1884, 25.

⁶⁴Report...1883, 23. Mitchell recorded, "How slowly women work into science" but her public comments were always encouraging: "If we cannot show rapid advancement of the culture of science among women, we can claim that there is slow and steady advancement." Report...1882, 25.

⁶⁵Mitchell apparently did not write the reports for 1878 and 1886, in the latter year because she was working hard to find support for the observatory at Vassar College. Mitchell to Antoinette Brown Blackwell, Aug. 22, 1886, Blackwell Family Mss, SLR.

⁶⁶Her concern with women in science did not diminish and her notebooks contain numerous clippings and anecdotes. Item 21 and others, MMM.

⁶⁷According to Alice Stone Blackwell, "Wherever it met it left behind a trail of new women's clubs and other organizations then considered novel and dangerous." Quoted in Inez Irwin, Angels and Amazons: A Hundred Years of American Women (Garden City, New Jersey, 1934), 229. In her Reminiscences Howe wrote, "Talk of the man with the hoe! We were the women of the plough. To us it was given to draw the furrow and drop the seed from which have sprung up clubs and federations scarcely to be numbered."

The General Federation of Women's Clubs was created as an alternative to the AAW and reflected a competition between the Boston area women involved with the New England Women's Club (which did not join the Federation when it was founded in 1889) and the New York Sorosis. The AAW persisted under Howe until "laid to rest" in 1900. Howe correspondence with Ednah Dow Cheney is particularly useful on these years. Jennie June Croly's The History of the Woman's Club Movement in America (New York, 1898) dismisses the AAW's two decades of work in one paragraph, calling it the "John the Baptist" of women's clubs.

After this paper was submitted, Karen Blair generously allowed the author to read her chapter, "The Association for the Advancement of Women: Domestic Feminism and Woman Suffrage" in "The Clubwoman as Feminist: The Women's Culture Club Movement in the United States, 1863-1914."

⁶⁸Winant, A History of Sorosis, 1868-1968, 20.

⁶⁹Robert Clarke, Ellen Swallow, The Woman Who Founded Ecology (Chicago, 1973), 86-90.

⁷⁰Mitchell conducted an officers' meeting of the AAW at the home of Phebe Hanaford and recorded with disappointment that most of the executive committee were women "of business" rather than learning. Common-place notebook, June 20, 1875, item 50, MMM. Mitchell read a paper to the collegiate group in Boston; see her copybook, Jan., 1880, item 50, MMM.

⁷¹Copybook, Aug. 27, 1879, item 50, MMM.

⁷²Her eye was always on the women who would succeed. She wrote in her diary that "the continuance of scientific study seems to be on the increase," with women more involved in serious study and "less apt to take up science as a pasttime"; Aug. 14, 1883, item 21, MMM.

⁷³She apparently had successfully opposed a presentation by Mary Livermore on writer Harriet Martineau. Livermore to Antionette Brown Blackwell, June 27, 1877, Blackwell Family Mss, Schlesinger Library, Radcliffe College.

⁷⁴Daughters of America; or Women of the Century (Augusta, Me., 1882); 252.

One of Maria Mitchell's students, Ellen Swallow Richards, was attracted to both astronomy and chemistry. Eventually chemistry won out, and she turned her talents to graduate work in the chemistry laboratory at Massachusetts Institute of Technology. Richards' skill in chemistry led her to make advances in many fields: sanitation, food purity, metallurgy, and industrial safety. This article stresses the contributions Richards made in the field of public health although she is perhaps best known for her application of chemical research techniques to household problems, thus initiating the field of home economics. This article describes how Richards fostered the chemical education of a whole group of women at the Massachusetts Institute of Technology.

ELLEN H. RICHARDS (1842-1911), SANITARY CHEMIST AND PIONEER
OF PROFESSIONAL EQUALITY FOR WOMEN IN HEALTH SCIENCE*

by George Rosen

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When the first volume of the American Journal of Public Health appeared in 1911 it contained the obituary of Ellen H. Richards who had died that year. Today probably very few readers of the Journal would recognize the name of Mrs. Richards, or know where she belongs in the history of American public health. Ellen Richards has not been ignored by historians of public health, but her role as a pioneer in bringing women into community health activity has been insufficiently emphasized. [See the list of references for publications that do make mention of Richards.] Consequently, at a time when there is a heightened awareness of the roles and contributions of women in American society, it is appropriate to recall the career of a woman who was a pioneer in public health and in the education of women.

Ellen Henrietta Swallow was born on December 3, 1842, near the village of Dunstable in northern Massachusetts, the only child of Peter and Fanny Swallow, both members of families already settled in New England in the 17th century. Ellen Swallow was educated informally at home, and also attended the local academy where she was taught some mathematics, English composition, French, and a great deal of Latin. At home she learned the

*All biographical information was taken from Caroline L. Hunt, The Life of Ellen H. Richards (Boston: Whitcomb and Barrows, 1912).

skills of household management, and by helping in her father's store also acquired a practical understanding in handling financial matters.

An unhappy love affair led Ellen Swallow to consider further education, but owing to the absence of a local college for women, it was not until September, 1868, that she entered Vassar College. Classified as a special student because of her advanced preparation, she was admitted to the senior class about a year later and graduated in 1870. At Vassar, Ellen Swallow was strongly influenced by two members of the faculty, C.A. Farrar, head of the Department of Natural Sciences and Mathematics, and Maria Mitchell, professor of astronomy. Influenced by the latter to study science, it was the former's position that science should be applied to the solution of practical problems that led her to study chemistry.

Upon graduation, Ellen Swallow was faced by the need to earn her living, and like so many educated women of the time in a similar situation she turned to teaching. As one of six teachers hired by President Sarmiento of Argentina, she was prepared to go to South America, but the arrangement failed because of unsettled conditions in that country. At this juncture she finally decided to go ahead with her plan to study chemistry, and applied to several commercial chemists for a position as an apprentice. Her application was rejected, but she was advised to try to enter the Massachusetts Institute of Technology, which had been opened in 1865, the same year that Vassar opened. Prior to 1871 the students were all men, but in January of that year Ellen Swallow was admitted as a special student in chemistry. She thus became the first woman to enter the Massachusetts Institute of Technology, or, for that matter, any school of science in the United States. The faculty considered her admission "in the nature of an experiment," and although President Runkle had worked for her admission her fees were waived so that in case any of the trustees or students objected to her presence he could say she was not a student. Despite these hesitations, she soon established herself as an able student, and gained acceptance by both the faculty and her fellow students. In 1873, she received the degree of Bachelor of Science (in Chemistry), thus becoming the first woman graduate of the Massachusetts Institute of Technology. That same year she was awarded the Master's degree from Vassar on the basis of a thesis and an examination.

During the four years from 1871 to 1875 Ellen Swallow was active first as student, then as student assistant, and finally as assistant in the chemical laboratories. It was during this period that she first came into contact with public health work. On April 6, 1869, the Massachusetts legislature had passed an act to establish a State Board of Health. Soon after its organization the Board took up the problem of stream pollution and requested Professor William R. Nichols of the Massachusetts Institute of Technology to make the necessary chemical analyses. As a result of his preliminary report, the Board in 1872 undertook a more extensive study of the pollution problem as related to sewerage and water supply. Nichols was again chosen to carry out the chemical work and this time he chose Ellen Swallow to be his assistant. This assignment brought her into public health work, and specifically into environmental sanitation, which was then an area of major activity. In 1884 the Massachusetts Institute of Technology opened a separate laboratory for sanitary chemistry with Nichols in charge and Ellen

Swallow Richards (she had married in 1875) as assistant. Nichols died in 1886 and was succeeded by Thomas M. Drown as professor of chemistry. That year the Board of Health established the Lawrence Experiment Station to investigate the principles involved in the purification of sewage-polluted water, to determine the tolerable limits of pollution, and to study means of achieving pure water and dealing with sewage. The following year an extensive sanitary survey of the waters of Massachusetts was initiated, which lasted 2 years. To a very considerable degree, the success of this enterprise depended on the laboratory work. In the course of the investigation more than 40,000 samples were analyzed, so that the success of this enterprise depended largely on the organization and the quality of the laboratory work. Ellen Richards analyzed many of the samples, either wholly or in part, and administered the laboratory with careful attention to the needs of the survey. As a result the final report to the Board of Health noted that "the accuracy of the work and the no less important accuracy of the records were mainly due to Mrs. Richards' great zeal and vigilance." (1) Thereafter she remained in charge of the water laboratory of the Massachusetts Board of Health until it moved to the State House in 1897.

After her initial experience in water analysis under Nichols, Mrs. Richards acquired a private practice in sanitary chemistry, including the analysis not only of water but also of air and food as well as testing of wall papers and fabrics for arsenic. In 1878-1879 she examined a large number of groceries for the state of Massachusetts, and the results of her investigations were published in the first annual report of the Board of Health, Lunacy and Charity which had replaced the first Board of Health.

While still a student, in addition to working with the professor of chemistry, Mrs. Richards had also assisted Professor Ordway, an industrial chemist, who did a great deal of consulting for various manufacturers. Through this association, in 1884, she was appointed chemist for the Manufacturers' Mutual Fire Insurance Company. In this capacity she performed valuable work on the spontaneous combustion of oils in commercial use. After 1875 Mrs. Richards also worked on metallurgical problems connected with mining, in large measure as a consequence of her marriage that year to Robert H. Richards, head of the Department of Mining Engineering at the Massachusetts Institute of Technology. Worth noting is that on their wedding trip to Nova Scotia they were accompanied by Professor Richards' class in mining engineering which he was taking along for practical field work.

Throughout this period Mrs. Richards was consolidating her position as a member of the Institute's staff, and in 1884 she was appointed Instructor in Sanitary Chemistry, a position which she held for 27 years until her death. For her students in sanitary chemistry she wrote Air, Water and Food; From a Sanitary Standpoint, in collaboration with Alpheus G. Woodman. In 1890, the Institute inaugurated the first systematic course in sanitary engineering and Mrs. Richards was actively involved. She taught analysis of water and sewage, as well as air analysis in relation to ventilation.

As a teacher, Mrs. Richards was instrumental in training a large number of young men to be sanitary engineers and chemists, yet she was never promoted to professorial rank. Moreover, her efforts to provide similar instruction for women had to be developed outside the regular curriculum of the Institute, although some assistance was given. In 1875, Mrs. Richards urged the Woman's Education Association of Boston to provide money for books, instruments, and apparatus, if the Institute of Technology provided space. Agreement was reached on this basis, and in 1876 the Woman's Laboratory was opened in a small frame building on the grounds of the Institute. The new laboratory was placed in charge of Professor John M. Ordway and Mrs. Richards was appointed as his assistant with the title Instructor in Chemistry. By April, 1877, 23 students, most of them teachers, had been admitted to the laboratory. Over the following 7 years, Mrs. Richards devoted a large part of her time and effort to the laboratory, working without salary and contributing about \$1,000 annually for its support. Equally important was the example she set for the students and the help she secured for those who had financial or health problems, even taking students into her home when necessary.

Once the Woman's Laboratory was firmly established, Mrs. Richards tended to direct the attention of the students toward problems which she considered of special concern to women--such as the analysis of foods--since she believed strongly that women as consumers should be informed on such matters. As part of this effort she published two small books, The Chemistry of Cooking and Cleaning (1881) and Food Materials and Their Adulteration (1885), which went through several editions. As she noted in the preface to the third edition of the latter volume published in 1906, "The original aim of the author and her collaborators was to arouse women providers for their families to the need of a study of the materials they purchased, both from a sanitary and economic point of view. . . . To the housewife and mother we say: For the sake of your children keep yourselves informed of the true state of the food manufacture. Do not accept all sensational headlines, but yourself study and give your daughters an opportunity to study chemistry in the high school. . . . It will take time and thought, but on what can these be better spent than on that which gives health and vigor for the better enjoyment of all the good things of life?" (2)

In 1883, the Woman's Laboratory building was torn down and the need for Mrs. Richards to supervise this unit ended. Several years earlier, in 1878, the Massachusetts Institute of Technology had decided to admit women on the same basis as men. Consequently, after 1882 when a new chemical laboratory was erected, facilities were included for women students. Thereafter, Mrs. Richards (as mentioned previously) was appointed Instructor in Sanitary Chemistry in the Institute's faculty, also performing the duties of a Dean of Women although she was never given the title, apparently because of her position that "a Dean of Women is out of place in a coeducational institution." (3)

Mrs. Richards, like so many social pioneers of the period, believed strongly in the efficacy of education, an attitude clearly evident in her effort to influence the worker's choice of diet through the New England

Kitchen which opened in Boston on January 1, 1890. This enterprise was undertaken together with Mrs. Mary Hinman Abel, whom Mrs. Richards had met in 1889 at the meeting of the American Public Health Association in Brooklyn, NY. The preceding year, in the APHA Lomb Prize competition for the best essay on "sanitary and economic cooking adapted to persons of moderate and small means," Mrs. Abel's essay on "The Five Food Principles Illustrated by Practical Recipes" had been judged best by a committee of which Mrs. Richards was a member. Financed by Mrs. Quincy Shaw of Boston to make "a thorough study of the food and nutrition of working men and its possible relation to the question of the use of intoxicating liquors," and by Edward A. Atkinson, a textile industrialist, interested in "pecuniary economy of food," that is, in seeing that the consumer obtained his dietary needs in the most economical manner consistent with good health, the New England Kitchen attempted to achieve these objectives.

Although the Kitchen did not succeed in persuading the poor of the advantage of low priced, nourishing food as prepared there, the efforts of Mrs. Richards and Mrs. Abel did lead to the introduction of school lunches into the schools of Boston and of other parts of the United States. Furthermore, through her work in nutrition, Mrs. Richards became associated with the studies on the subject of the United States Department of Agriculture. In 1893, Atkinson advocated the establishment of food laboratories as part of the Agricultural Experiment Stations created several years before, and, in 1894, W.O. Atwater, director of the Office of Experimental Stations, began to investigate the nutritive value and digestibility of various foods and to survey the diets of various groups in the population. The first technical bulletin issued as a result of these investigations was entitled Methods and Results of Investigations on the Chemistry and Economy of Food (Bulletin 21, Office of Experimental Stations). An important section summarizes studies by Mrs. Richards and Mrs. Abel on various foods and methods of preparing them. Under the same auspices, Mrs. Richards (with Amelia Shapleigh) prepared a paper on "Dietary Studies in Philadelphia and Chicago, 1892-1893," reporting the results of a survey of food consumption and dietary habits of low income families. Attention should also be drawn to her efforts to improve the quality of institutional food.

After her marriage to Professor Richards, she combined homemaking with her professional work, becoming a pioneer in the simplification and systematization of housework. This was a practical matter for Mrs. Richards personally, but as her interests developed she became firmly convinced that women had to obtain control over the conditions of their lives so as to free themselves for broader living. Efficiency in house-keeping was simply a means toward this end, an aim that could best be achieved if education for this purpose, a fuller life, began at an early age. She saw the introduction of science teaching in the public schools as one aspect of this process. Furthermore, she believed that teaching women to handle their problems was beneficial to the entire community because they would be educated citizens. To further this purpose Mrs. Richards published a number of books, among them The Cost of Living as Modified by Sanitary Science (1899, 1905) and The Cost of Cleanness: A Twentieth Century Problem (1908). Increasingly, the last 30 years of her

life were given to the development of what she called euthenics, "the science of the controlled environment," of which efficient management of the home was a part.

Keenly interested in advancing professional opportunities for women, she also believed that a happy married life was desirable and possible. But she was also firmly convinced that women need not lose their individuality in marriage. "It is becoming recognized," she wrote, "that woman has a personality that is not in her husband's control, that the mere fact of marrying him does not make her his devoted slave." (4)

Throughout her life Ellen H. Richards acted to advance the health of the community and the position of women in it, especially with respect to education, occupation, and circumstances of life. Some of her goals were attained, others still remain to be achieved, but she was a pioneer who merits continuing recognition by all community health workers.

Footnotes

¹C. L. Hunt, The Life of Ellen H. Richards (Boston: Whitcomb and Barrows, 1912), p. 103.

²E. H. Richards, Food Materials and Their Adulterations 3rd ed. (Boston: Whitcomb and Barrows, 1906).

³Hunt, p. 151.

⁴Ibid., pp. 321-322.

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Integration

The early colleges placed little emphasis on academic degrees for faculty members. Both men's and women's colleges required only knowledge of certain subjects, and sometimes very little even of that. Thus Maria Mitchell's eminence as an astronomer more than qualified her for her post at Vassar, although she had received no formal higher education. Advanced degrees rapidly came to be required for college educators, and women's colleges had an especially difficult time finding female Ph.D.'s to fill academic posts.

Although not mentioned by Rosen in the preceding article, Ellen Swallow Richards was not allowed to receive a doctorate from M.I.T. Her husband believed that this was because M.I.T. did not want its first Ph.D. in chemistry to go to a woman (Clarke, 1973). Because Ellen Swallow Richard's experience was not unique, women's educational associations began funding European fellowships that enabled women to obtain Ph.D.'s and M.D.'s abroad.

American men had been going to Europe for their educations in increasingly large numbers from the 1850s on. Women were first permitted to study for advanced degrees in Switzerland in the 1870s, and gradually the other European countries followed suit. Some women studied abroad because it was impossible for them to obtain a doctorate in their chosen field in America; others went because a European education, especially in many scientific fields, was thought to be far superior to an equivalent education in the United States.

The article "Before Women Were Human Beings..." is the firsthand account of Ida H. Hyde, one of the early recipients of a European fellowship. The article describes the difficulties she encountered; however, Ida was quite used to overcoming difficulties. When she was sixteen, she was apprenticed to a millinery establishment to supplement her family's finances. For several years, she attended night school each evening after work. At age 23 she had saved enough money to attend college for one year. Next she taught school for eight years before completing her pre-medical degree at Cornell. Ida Hyde was a graduate student in zoology at Bryn Mawr for two years prior to receiving the fellowship to study abroad. After she returned to America, Dr. Hyde conducted research at Harvard, as she describes in the article. In 1899 she moved to Kansas University where she founded the Department of Physiology and served as a faculty member for two decades.

BEFORE WOMEN WERE HUMAN BEINGS . . . ADVENTURES OF AN
AMERICAN FELLOW IN GERMAN UNIVERSITIES OF THE '90'S

by Ida H. Hyde

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Early in the last decade of the nineteenth century, a polemic between two European professors pertaining to the development of an organism they were investigating led to bitter personal criticisms that finally appeared in print. The controversy aroused the interest of embryologists in this country, particularly a student in Bryn Mawr College, who without knowing of these professors was conducting experiments on the very problem about which the dispute centered.

The results obtained by the student in her investigation corroborated those published by one of the disputants, Professor Goette of the University of Strassburg. When Goette was informed of this fact he was very much elated. Eager to have his interpretation of the results strengthened and the investigation of the problem variously extended, he invited the student to come to the University of Strassburg and continue study of the subject in his department.

This invitation came to me as a complete surprise. Unfortunately it seemed impossible at the time to accept the tempting suggestion. But suddenly a way was unexpectedly opened through the splendid offer of the European Fellowship awarded in 1893 by the Association of Collegiate Alumnae for study in foreign universities. Thus in a short space of time and in a most extraordinary manner the realization of the dream to work in the promised halls of Strassburg University became a reality.

At the time the European Fellowship was awarded, it was not known to my professors nor to me that universities in Germany were not coeducational institutions, and that women had never studied in the University of Strassburg, in fact, that they had not been permitted to matriculate in any German university. Therefore we on this side did not appreciate the full significance and importance of the departure, when Professor Goette, director of the Zoology Department in the University of Strassburg, graciously invited a woman student of Bryn Mawr College to work in his department.

It was not until I had worked many days in the splendid laboratory assigned to my private use that it dawned upon me that I was occupying a unique position, and that I was regarded by the students, faculty members, and their wives as a curiosity. In the university circle the news quickly spread that an American "woman's rights" freak, a bluestocking and what not, had had the boldness and audacity to force entrance into the college halls. At Kaffee Klatchen she was served for gossip and dissection.

It was not unusual for a professor, student, or diener, seemingly by mistake, to open the laboratory door, look frightened, and quickly retreat. Or students would congregate at the windows of the botanical building opposite the laboratory, and from sheer curiosity stare across at my windows, greatly to the annoyance of the professors in both buildings.

My hostess, the wife of a professor of mathematics, occasionally invited me to accompany her to social affairs. At a dinner that we attended I met the charming wife of Professor Goltz, one of the most distinguished physiologists in Europe. Frau Professor Goltz was deeply interested in learning of the great independence enjoyed by women and women students in America. In the course of the conversation I remarked that I had specialized in physiology and had been assistant to and conducted investigations under Dr. Jacques Loeb, one of the Professor Goltz's former assistants, from whom I had a letter of introduction to her husband. I ventured to inquire whether she thought Professor Goltz would allow me to work in his department. My heart sank when she replied that her husband was bitterly opposed to the admission of women to the Physiological Institute, the more so because it belonged to the Medical School, where women were taboo. However, she arranged for a meeting in her home, when the letter of introduction might be presented.

At the appointed time I found myself in the library of Professor Goltz's home, waiting for the eminent professor with little hope of success. Soon he appeared, followed by a fierce looking bulldog that greeted me with growls and terrifying barks. The stern, dignified professor attempted to calm the beast, and with strained gestures of welcome motioned me to a chair. He read the letter, apologized for the animal's behavior by informing me that the dog disliked women, and regretted that in spite of his high esteem for Dr. Loeb and the recommendations he could not admit me to the Physiology Department because the medical students would resent the presence of women there. However, he would be pleased on Sunday afternoon, following the dinner to which his wife had invited me, to show me the results of some experiments that his associate and he had recently conducted.

I reported the discouraging interview to Goette and to my host and hostess, who promised to speak in my behalf to Professor Goltz and to his associate, Professor Richard Ewald. The latter and his wife were also guests at the Sunday dinner, and accompanied us to the Physiological Institute.

When I saw the well equipped laboratories, museums, demonstration and preparation rooms, and remarkable experiments that had gained international renown, my interest and enthusiasm were deeply aroused. As we were leaving the building, I told Professor Ewald it was unfortunate that I was not a man with the privilege of working in his department. He assured me that there would be no objection to my watching experiments conducted in his private laboratory.

The following day, to my great delight, he invited me to witness the delicate technique required in experiments on the inner ear. Several times

thereafter on or two of his research students and I were visitors at his demonstrations. I took pains to make a thorough study of the anatomy and technique involved, and to my surprise was rewarded by an invitation from Professor Ewald to cooperate in an investigation of the brain's relation to the peripheral center of sound--a study which was later published over our joint signatures.

Needless to say, I was overjoyed at the prospect of working in a physiological research laboratory where opportunity was offered to acquire valuable technique and experience in unusual operations. Now my time was divided between work in the zoological and physiological laboratories. Professor Goette was satisfied with the results obtained on the problem in which he was interested, and to my great surprise offered to accept my investigation for a doctor's thesis. Furthermore, he advised me to petition the Ministerium of Education in the Reichstag to permit the faculty of the University of Strassburg to allow me to work for the doctorate. It was necessary to obtain the consent of that august body and the approval of the faculty, because the constitution of the university did not contain a clause permitting women to take the examination for the advanced degree.

Before petitioning the Reichstag, however, it was deemed advisable to ascertain the attitude of the Strassburg faculty regarding the question of giving women the privilege of taking the examination. My petition, accompanied by many influential credentials, was presented for faculty action at the November meeting. It aroused heated discussion, and Professor Graf Solms, the director of the Botanical Department, announced that as chairman of the examining commission he refused to allow a woman to take the examination.

Believing that the disagreement aroused in the faculty by Graf Solms's attitude might prejudice the cause of woman students in Germany, I hastened to beg that my petition be withdrawn. This step, it proved, gained friends and no enemies for the cause.

When an American friend, engaged in publishing her late husband's manuscripts with the help of Professor Rosenbusch, the noted geologist in the University of Heidelberg, heard of Graf Solms's action, she urged me to try to obtain the degree in Heidelberg. But first I wished assurance that the university would actually grant me the degree. Accordingly a formal request was dispatched in November 1893 to the Reichstag's Ministerium of Education, "that universities in Germany permit women students, prepared in collegiate required subjects as thoroughly as were men, to matriculate, take the examination, and if successful in passing it, to receive the doctor's degree." It is noteworthy that this petition to the Reichstag pertained not only to my cause but to that of women in general. With this carefully prepared document were enclosed many credentials from noted educators and scientists.

After several weeks of waiting, a formidable-looking document from the Reichstag Ministerium arrived. It informed me that the petition had been presented to the High Commissioner of Education, and its object duly incorporated in the minutes of the day's proceedings. Furthermore, it advised

that, inasmuch as I was hoping to enter the Heidelberg University, my petition should be sent to the Ministerium of Education and Justice in the Duchy of Baden, under whose jurisdiction the University of Heidelberg was governed. Also, my petition must receive the approval of the Grand Duke of Baden, who was rector of the university.

In considering the situation, it seemed best to finish the zoological and physiological investigations, and prepare them for publication before leaving Strassburg. In the meantime I addressed a letter to Professor Geheimrath Stengel, Dean of the Faculty of Natural Sciences and Mathematics of the University of Heidelberg, asking if the university would give me the degree of Ph.D. provided I presented a satisfactory thesis, pursued the required subjects in the university for the length of time deemed necessary, and passed the examination for the degree. This letter marked the beginning of correspondence that extended over a period of two years. The story may be briefly told here.

A most interesting letter, dated December 9, 1893, was received from Dean Stengel. He informed me that up to date no woman had petitioned the Natural Sciences and Mathematics Faculty for permission to matriculate and to take the examination for the doctorate. Furthermore, that there was in existence no record or constitutional clause stating that women shall or shall not be allowed to prepare for and take the examination. However, to obtain an official decision, a special printed blank, petitioning for the privilege of taking the test, must be executed and presented for faculty action.

It was so very important that this petition should receive a favorable vote from the faculty that I decided to go to Heidelberg to obtain an audience with the professors under whom I wished to study, and if possible secure their support. Therefore I wrote Professor Butschli (Hofrath Geheimrath Professor Doctor Otto Butschli), the renowned scientist and director of the Zoological Institute of the university, and begged for an interview during the Christmas vacation. This favor he kindly granted.

Arriving at the appointed hour, armed with my thesis, drawings, preparation-slides, and letters, I was shown by the diener to the professor's private laboratory. We found him bending over a microscope, absorbed in studying an object under it. With many apologies the diener announced a woman wished to speak to him, and inquired if she should be admitted. Turning his heavily bewhiskered face, and scanning me with searching eyes, he ejaculated, "Ja wohl. What can I do for you?" Apologizing for the interruption and thanking him for the granted interview, I gave him Professor Goette's letter of introduction. Then I inquired if he would kindly examine the manuscript and illustrations, and let me know if I might work in his department toward a Ph.D. He asked me to leave my papers, saying that I might expect word from him in a few days.

Upon Professor Butschli's decision now depended my hope of entering the university. When on December 27, the diener brought my manuscript and letter, I opened the message with trembling fingers. Imagine my surprise and delight! Not only would he recommend the thesis for the degree,

but he was also willing to enroll me with student's privileges in his department.

With such a delightful prospect, I dispatched the official petition blank to the Dean on January 11, 1894, to be presented to the faculty. Accompanying the petition were all of my letters and credentials and a personal letter in which I begged to be informed whether there existed any faculty ruling against admitting women to the examination for the doctor's degree.

A special meeting of the faculty was called to consider the petition. All members were present and took active part in the prolonged discussion. It was recognized that the questions asked in Miss Hyde's petition were for the first time in the history of the university presented for definite official action. After much discussion, two motions were voted:

- (1) Women are admitted to the examination for the degree, under the same conditions outlined in the official regulations for men candidates, with the proviso that women are required to study in the University of Heidelberg in preparation for the examination.
- (2) A faculty rule does not exist against admitting women candidates to the examination for the doctor's degree.

Professor Quincke, professor of physics, cast the only vote against this motion. Further discussion finally influenced him to join the majority, making the vote unanimous.

The meeting also voted to petition the Senate to present the voted resolutions to the legislative and executive bodies for approval. On February 26 the Dean was notified that the resolutions adopted by the faculty were approved both by the Legislative Ministry and the Senate, with the proviso that in granting this petition it was understood that a precedent was not established justifying women in demanding admission to the university, since it had not as yet in any wise been decreed or announced by the superior Ducal Commission that women should be admitted to Baden's university on equal terms with men.

Shortly thereafter a letter bearing the seal of royalty reached me. From it I learned that my petition and letters, through the general adjutant to his Highness, the Grand Duke of Baden, had received due consideration and favorable judgment.

It may be imagined with what profound gratitude and appreciation I received these tidings. They established for the first time in the history of this institution the significant and victorious outcome of the struggle to gain recognition for women candidates for the Ph.D. degree. As a consequence, women thereafter met few obstacles in entering any--except the medical--department in the University of Heidelberg.

Although equal opportunities with men students were not obtained, nevertheless a beginning was made toward that end. With the passage of

the decree any woman prepared to fulfill the university's requirements was permitted to study for the finals, and if successful obtain the doctor's degree.

In Strassburg, in the meantime, my physiological and zoological investigations were completed. On my arrival at Heidelberg much time was consumed in fulfilling the customary registration regulations, and in obeying the rules of etiquette. In accordance with the latter, I made a formal call on the Dean and asked for an opportunity to consult him on the subjects chosen for the finals. During the interview, granted late in March, he advised me to see the professors under whom I wished to study, obtain their consent to admit me to their departments and, when the time arrived, to conduct the examination. Furthermore, when all that had been attended to, I was to petition the faculty to approve the choice of subjects chosen, and beg it to invite the professors to be present at the examination.

The first steps taken in following the Dean's advice were to call on Professor Butschli, Director of the Comparative Anatomy and Zoology Departments, and Professor Victor Meyer, Director of the Chemistry Department. Both courteously welcomed me to their departments. However, they suggested taking some subject other than physiology, which I had chosen for the third subject required, since physiology was given in the Medical School, where women would not be admitted. Indeed, Geheimrath Kühne, professor of physiology, had recently announced in a public lecture that women's place was in the home and not in the university. They advised securing an interview with him at once, since the presemester faculty meeting, to which my petition must be submitted for approval, was scheduled for the following day.

Without further delay I called on Professor Kühne and presented a letter of introduction from his friend and colleague, Professor Goltz of Strassburg. He seemed a giant, seated in a huge armchair at a table in a spacious library. Without rising he expressed pleasure in receiving the letter and meeting me. When I told him that I was greatly interested in physiology, he kindly offered the use of his library, and asked what books he might send to my address. He expressed approval in the choice of books.

I then told him that I expected to work in the departments of Professors Butschli and Meyer, and wished to know if, after physiology had been sufficiently mastered, he would examine me in that subject for the degree. He appeared to regard the question as a joke, and laughingly said, "Certainly, if that time should ever come." As he rose to show me out, he casually remarked that his conference hours were Saturday morning in his laboratory, where the books might be returned and where he would be pleased to discuss them with me.

I hastened to tell Professor Meyer the result of the interview. He expressed great surprise, and assured me that since Professor Kühne had given his promise, he was obliged to fulfill it. He also aided me in preparing the petition that was to be presented the following day for faculty action.

It was uncertain what attitude the members of the council would assume to the proposal to allow a subject belonging to the Medical School to be credited by the natural sciences and mathematics section as one of the three required subjects for the Ph.D. However, Professor Meyer reported after the meeting that finally a favorable decision had been reached.

But at the council meeting Professor Kühne had announced that he must be excused from giving the examination, and furthermore that he refused to let "skirts" enter his lecture room or laboratory. When questioned whether he had not promised to conduct the examination, if I should ever be prepared for it, he had admitted making that statement, in reply to what he considered a joke. He had had no idea that he was being made the victim of an American scheme in order to secure his favorable answer. To the statement that there was no proof that unfair methods had been employed or that the question had been put as a joke, he had replied that since it appeared that he ought to adhere to his promise, "as a gentleman he would keep his word." But without admission to the lecture room or laboratory, how could one master the subject sufficiently to pass the finals?

Even with this disappointment, the decision of the faculty meeting was more than I had expected. Was there not sufficient cause for rejoicing in the knowledge that a long stretch of the difficult pioneer route on the higher education of women had been traversed? The taboo chasm of tradition and prejudice that defied the aspirations and attempts of earnest women to reach the mecca of intellectual freedom had been bridged and actually crossed to the sacred road beyond leading to the open door of the seat of learning.

At an early hour of a spring morning in 1894, I entered the new Zoological Institute of the University of Heidelberg, selected a back seat in the lecture room, and was assigned to a well equipped laboratory. Over the table I hung a picture of the Grand Duke of Baden, writing below it "Dankbarkeit erzougt Ehrfurcht und Gehorsamkeit,"--an answer to the fear expressed by the Duke, that "if women became better educated, they would no longer respect, honor, and obey men, as they should." (The picture still hung there when I visited the laboratory thirty years later.)

In the Chemistry Department I was greeted by Professor Meyer, who placed my name on one of the most desirable places, where the experiments and demonstrations could most readily be observed.

Seemingly all was going well. But how to approach Kühne without making an irretrievable mistake was a question that constantly haunted me. What if he should refuse to admit me to the lectures and laboratory courses in physiology? How would it be possible without attending them to prepare for the finals?

It is true, botany could be chosen as a third subject, and much heartache avoided. But I preferred physiology above every other subject, and had determined to specialize in it. To abandon that decision forever

seemed to me a calamity. There was but one thing to do: study the books that Kühne had loaned me, and take them to the laboratory where on Saturday morning he held his seminar.

When the diener heard my request, he grinned most politely and led the way to the conference room. That gave me courage. Professor Kühne greeted me with extreme politeness. That greatly disturbed me. He questioned me on subjects of investigations that had recently appeared in foreign publications, and on every subject except those dealt with in the books. Being unprepared for the attacks he successfully made to dishearten me, I ventured to ask the loan of literature about which he had quizzed me, and told him I regretted that the topics treated in the books had not been discussed. Thereupon he politely assured me that the examination might also deal with questions with which I was unprepared, and that as a matter of fact only a small per cent of the men students successfully passed the examination in physiology. Finally he advised registering [in] the botany.

I replied that physiology interested me more than any other subject and if necessary I would devote a year or more in further study to master it, and asked if it were not possible to accomplish this with the aid of his assistants. He laughingly replied that his assistants had never attended his lectures; therefore it was doubtful they would be able to help me. He offered me a treatise on the physiology of blood, with the remark he would be interested in discussing the subject matter with me any Saturday morning.

During the ensuing weeks most of my thoughts were stained in blood. But when I came to the conference and begged to report on the topics dealt with in the books, Kühne, seemingly annoyed, inquired if I had heard that the professor of botany would admit me to his department. I replied that I had come to Heidelberg to study physiology and had just received word that the Phoebe Hearst fellowship for study abroad had been awarded me. I therefore felt in duty bound to make a success of my work in the university, and hoped he would assist me to that end.

I was dumbfounded when he rose, opened the door to the general laboratory, and beckoned to his assistants. He introduced me with the remark that I was determined to study physiology, and since he refused to let me attend his lectures, perhaps they would undertake to help me attain my object. They said that with his consent they were willing to try.

On the following morning, the medical students who crowded the physiology lecture room wondered why the two chief assistants were occupying the front seats. This was the first time in the history of his instruction that Kühne was honored by the presence of his associates in the lecture room. To his amazement and satisfaction they were actually taking notes.

The notes were taken for me, to be copied for study in preparation for the finals. Six hours daily were devoted to the subject. In the opinion of the instructors, this schedule followed for at most six semesters should prepare me for the most rigid examination.

During these months of arduous work, the thought haunted me that Kühne would do all in his power to frustrate my purpose. He bitterly resented the gossip aroused among his colleagues by the stand his assistants had taken, and he resented being an unwilling agent in permitting "skirts" to enter the Medical School.

Finally the morning of the first lecture in chemistry arrived. On reaching the building, I was dismayed to hear an uproar within, and awakened to the realization that I was late. The door of the lecture room was closed. As I stood before it, the chills ran up and down me, and my courage failed. It was impossible for me to face the excited crowd of noisy students. Turning to leave as quickly as possible, I saw students rushing upstairs. I realized that if I failed then it would require more courage to enter the classroom the next day. The students jostled each other and hastened by, leaving me standing in the open doorway.

A silence followed, so profound you could hear a pin drop. The men stood seemingly transfixed in their various attitudes. I never knew how I got to my seat. The blood was rushing to my head, and in the hush I distinctly heard an American voice say, "We shall next have them in the jury box."

To hear those taunting words in my profound embarrassment from an American would have proved too disheartening if the remark had not been instantly censured with hisses and scraping of feet by the German students who heard it. And to the credit of the Heidelberg students it must be said that in all the time of my attendance at the university, they always treated me with the greatest courtesy.

For three semesters I attended the lectures and demonstrations in chemistry conscientiously. At the beginning of the second semester I no longer was the only woman student in chemistry. Encouraged by my report of the opportunities offered in chemistry and geology for advanced work, two of my Bryn Mawr College friends had come to Heidelberg for special studies.

The time spent in the Zoological Institute was most happily employed in research, attending lectures, demonstrations, and seminars. I shall forever cherish the memories of my sojourn there, as among the most delightful of my college days.

As a matriculated student at the University of Heidelberg I devoted two years in preparation for the doctorate toward which my researches and thesis had already been accepted and credited in the Natural Sciences and Mathematics Faculty. While working here it was gratifying to meet women graduates from Russia and America, who were now admitted under the finally established resolutions not only to the lectures but also to the laboratories.

Finally the last lecture and laboratory experiment came to an end. I began a review of the subjects, especially of physiology, the one that caused most worry, in preparation for the finals.

In accordance with university traditions, a visit to the Dean was in order for the purpose of asking him to present my request to the faculty to be permitted to take the examination, if possible in November. The faculty's favorable decision was sent to Kühne for concurrence. In reply he announced that owing to ill health he was leaving for Italy for an indefinite period.

This information was disheartening, not only because preparation for the tests was under way but also on account of the uncertainty of his return. Early in December Kühne appeared in Heidelberg. Again a formal visit to the Dean was necessary, and another petition was sent to the examining board asking at what date permission would be given to appear for the finals. The date was set for January 8, and Kühne was invited to conduct the test in physiology. He replied that the date was noted, and that he would be present.

While the process of cramming was going on, the weather became severely cold and damp. The change in temperature and lack of proper food and exercise no doubt contributed to a severe attack of neuralgia, and it was necessary to petition the Board to postpone the examination. This time it was set for 6 p.m. February 12, 1896.

At last the day arrived when in formal attire, according to custom, it behoved me to call upon and invite the professors to the examination. The invitation was graciously accepted, but Kühne said only that he hoped to be present. This implied uncertainty was very trying.

The morning of the momentous day found me in a wretched condition, unable to think or act. My loyal friend was deeply distressed. She feared that at the last moment my cherished hope was doomed to disappointment. It was late in the afternoon before I found it possible to take nourishment. By five o'clock we realized that I must either be in the university hall or dispatch word to the examining board that I was too ill to appear.

Suddenly an unaccountable consciousness of awakened power possessed me. Telling my friend that my indisposition had miraculously vanished, with her assistance I dressed and set out on foot to the university. The invigorating evening air and knowledge of recovered well-being imbued me with a spirit of confidence that stood in good stead.

The pedell in his elaborate uniform stood at the door. In a condescending tone he requested me to wait in the anteroom until he should be notified to conduct me into the hall. He stood erect holding his mace, it seemed to me an eternity, guarding the door when he ventured to say, "Geheimrath Kühne has not yet arrived." Suddenly the room was pervaded by an over-whelming chilliness. But presently the door was opened, and I was escorted to a chair at a long table surrounded by what appeared to me to be countless professors.

As I entered the room, wine glasses were being passed, and Kühne rose and handed one to me saying, "We shall celebrate this event with a

toast in champagne." In thanking him I humbly asked permission to drink water before and champagne to the toast after the examination. Laughingly they rose, and as the diener handed me the water, all drank to my success.

Professor Butschli placed his watch on the table, and Kühne as Dean of the Medical Faculty, had the honor of opening the quiz. He was keen in his questioning, and made no comments upon my answers. Suddenly Butschli pointed to the watch and inquired if he was aware that the allotted time had elapsed? Jocularly Kühne replied that in the enjoyment of the test he had entirely forgotten the time.

The examination lasted until ten o'clock when the pedell entered, showed me to the anteroom, and asked me to wait until the professors had left the hall. While adjusting my wraps, I heard discordant voices issuing from the hall, and feared the worst. When the pedell opened the door, some of the members, among them Butschli and lastly Kühne came toward me, extending their congratulations. The ordeal was over!

My friend was awaiting me in a cab at the entrance to the building. She kindly inquired if she might have my professors taken to their homes. Butschli accepted the offer, saying his wife was anxious to hear the result of the game, and to see me in the gown which she had heard I had made for the occasion.

The Frau Professor seemed so very pleased that I had come. Excitedly she asked her husband how Kühne had behaved. He answered that I had deserved Summa Cum Laude, but that the "brute" had objected to giving a woman that honor. Finally a compromise had been agreed upon, and a new term, Multa Cum Laude Superavit, with the title, Doctor of Philosophy and the Natural Sciences, was conferred upon me. Before I left, Frau Professor Butschli invited me to a dinner in my honor.

On the following day a great honor, an invitation to a dinner from Frau and Professor Kühne, was received. Most unfortunately, this was for the day set for the dinner of Frau and Professor Butschli, and could not be accepted. An invitation from the students, asking me to participate in a parade, was also declined. However, an enormous chocolate cake in the shape of a doctor's chapeau sent with congratulations was accepted most heartily.

I was the first woman to obtain the Ph.D. of Heidelberg not as a courtesy but as a bona fide university student in the Natural Sciences and Mathematics Faculty who had met in every detail the requirements of that institution's decree of March 7, 1894. The university, be it said to her honor, had established a precedent of far-reaching importance.

At this time two most astonishing letters reached me. The first stated that now, since I was a graduate of the university, the use of the Physiology Department was cordially placed at my disposal and Professor Kühne and his associates would be pleased to welcome me there. Shortly thereafter, the invitation was accepted, and Kühne's dictum that "skirts

should never enter his laboratory" had lost its significance. For the first time, a woman had been invited to do research in the Medical School of the University of Heidelberg. It was a pleasure to work in the research room with other investigators, to discuss experimental methods, and get new ideas on problems in physiology.

The second letter was an imposing official document informing me that the university contributed a sum equal to five hundred dollars to the Naples Marine Biological Laboratory in support of a research table allotted each year to an honor student, and that for the ensuing year, on the recommendation of Professor Kühne and the Naples Table Committee, the use of this research laboratory had been awarded to me.

This incomprehensible information dazed me beyond words. Never before had a German university allotted its Naples Table to a woman. It seemed as though Professor Kuhne was endeavoring to make amends for his harsh treatment.

Through the courtesy and splendid scientific and executive efficiency of Dr. Anton Dohrn, the director, his associates and famous general staff, an international laboratory of far-reaching scientific and educational benefits had been established on the bay of Naples. I was overjoyed at the prospect of working there.

In the spring of 1896 I began work in this largest and most important laboratory in the world. On the heights above the Station, Mount Vesuvius smoked, across the blue bay Capri smiled, in the adjoining city park the orchestra played gay tunes, and involuntarily the thought arose, "This is in truth a haven of bliss!"

What a rare privilege it was to pursue studies in this highly endowed station! Through the investigators who came here from all quarters of the globe it offered a center for interchange that led to international understanding and enduring friendships. At the end of three months the problem, "Physiology of Salivary Glands, Especially in Octopus," was finished under the direction of the gifted scientist, Dr. Schönlein.

Grateful for the generous spirit that pervaded all departments of the Station and the valuable benefits offered to men and women alike, I resolved upon returning to the United States to do all in my power to enable eligible women scientists to avail themselves of the laboratory's unexcelled opportunities.

When the results of the investigation on the salivary glands were sent to Professor Kühne, he acknowledged the report with an invitation that he had obtained for me from his colleague, Professor Kronecker, Director of the Physiology Department in the University of Bern. So I postponed my homeward journey until September, in order to accept Professor Kronecker's kind and tempting offer to be his guest in the institution and do some research that interested him.

The work in Bern under the genial director proved most interesting, and it is with deep appreciation and gratitude that I acknowledge my indebtedness for valuable assistance and suggestions.

Among the many visitors that I met in the laboratory was Professor Henry P. Bowditch, the distinguished physiologist of the Harvard Medical School. The problem I was working on, "Influence of Temperature on Muscle Activity," interested Professor Bowditch, and before he left Bern he kindly gave me a letter of introduction to his associate, Dr. William Townsend Porter, telling me that if Heidelberg and Bern opened their doors to women, there was no reason why Harvard Medical School should not admit them also.

There were good reasons for hastening to return to the United States. One very substantial one was that my funds were exhausted; another, that work must be done in the Harvard Medical School while there was a chance to enter.

I reached Cambridge in the autumn of 1896, and obtained part-time appointments to teach biology in three preparatory schools. In the afternoons I studied in the Harvard Zoology Department "The Nerve Distribution in the Eyes of Mollusca."

Later in the season I took Dr. Bowditch's letter of introduction to Dr. Porter, who kindly encouraged me to begin an investigation in the Physiology Department of the Medical School on the "Effects of Distention of the Ventricle on Blood Flow Through the Heart." (1) Under his masterly guidance, the research was completed and published in 1898 in the American Journal of Physiology. It was read before the American Physiological Society, in which association I had later the honor of being elected a member--one of the first, if not the first, woman to be chosen.

In the midst of my research I had not forgotten my purpose, to provide the opportunity for American women to work at the Zoological Station where I had been so generously welcomed. At a dinner in Cambridge for a group of influential women, mostly educators, I suggested that an organization be created to support a table for American women at the Station. The idea met with unanimous approval, and as temporary secretary, my wholehearted energy was put in the task of arousing interest and support for the organization among educational institutions and scientific societies in this country.

The organization was eventually enlarged and given the title, "The Naples Table Association for Promoting Research by Woman." Among the founders were President M. Carey Thomas, Professor Ellen Richards, Mrs. Alice Freeman Palmer, President Irvine, Dean Agnes Irwin, Miss Agnes Longfellow, Miss Florence Cushing, Miss Gill, Mrs. Elizabeth Clarke, and myself. It was composed of thirteen colleges, two associations, and five individual contributors, each contributing fifty dollars annually. It later also offered research prizes of one thousand and then two thousand dollars, to encourage women in individual research.

Reviewing all these developments, is it too far-fetched to accord Professor Kühne the credit for the rare opportunities given me--to work in the Naples Station, in the University of Bern, and the Physiology Department of the Harvard Medical School? If not, then "All is well that ends well."

But not without credit given to the American Association of University Women. To the Association belongs the honor and any success that has been achieved, through its establishment of women's fellowships. Without the opportunities given me, through the Association's fellowship, and the Phoebe Hearst award, with which the Association was also concerned, I should not have been able to contribute my share, although a small one, to securing for women the privileges of university education enjoyed by men.

Footnote

¹This was the first time, as far as I have been able to learn, that a woman carried on research in the Harvard Medical School.

The next article clearly illustrates how the percentage of women earning bachelor's and advanced degrees has been influenced by historical periods such as the Depression, the post-World War II era, and the second wave of feminism that began in the 1960s.

STRIVING FOR EQUALITY: HIGHER EDUCATION

FOR WOMEN IN THE U. S. SINCE 1900

by Joan N. Burstyn

Reprinted from University College Quarterly 18:2 (January 1973): 22-32.

Any discussion of trends in higher education for women is indirectly a discussion of women's role in the professions. While the twentieth century has witnessed the professionalization of many occupations, the percentage of professionals who are women has declined since 1930. (1) Recent moves to appoint more women to administrative and senior positions in the professions, government, industry, and the universities may stop this decline, but any major increase in the percentage of professionals who are women is unlikely so long as work patterns remain unchanged. Nearly ten years ago Bruno Bettelheim claimed that efficiency had become a fetish in our society. (2) Efficiency in professions has dictated that professionals shall not relax into non-professional routine even for a few minutes--medical assistants arrange patients, ready-stripped and documented, for doctors to make decisions on without pause; secretaries prepare dossiers for executives to act on. Rhythmic change in the performance of men and women has to be eliminated; people have to perform like machines. Bettelheim suggested that our wealth could enable us to drive ourselves less hard. "We could do it by making allowances for the need to humanize work; by making it companionable, unexhausting, diversified, and satisfying in itself, beyond the wages it brings." But there is no sign that professionals are prepared to reassess the nature of their work in order to humanize it; and until they do so, women who aspire to professional careers will face the same tensions that professional women have faced for many years.

At the turn of the century educated women were expected to quit work once they married. Many chose deliberately to eschew marriage for the sake of a career. (There was confusion in their ranks when Alice Freeman, president of Wellesley College, chose marriage instead. Apparently neither she, her husband, nor anyone else, could envisage her carrying on as President once married.) However, the number of women graduates in 1900 was small. Of a total of 27,410 bachelor's and first professional degrees awarded that year, only 5,237 went to women. (3) This figure was double the number of degrees awarded to women a decade earlier, yet the percentage of women graduates marrying in the decades preceding 1900 remained fairly stable at about 50%. A majority of those

women who chose to marry waited five to ten years after graduating before they did so; in the interim they put their education to use in such careers as teaching, medicine, and social work. The rest of the women graduates, almost one half, resolved their "marriage-career conflict" by repressing or sublimating sexual drives for the sake of their careers. However, there were never many women willing to do that, and their numbers have not increased dramatically although the number of women awarded bachelor's and first professional degrees in any one year has risen from about 5,000 in 1900 to 224,491 in 1966. By the 1960s it was estimated that well over 90% of women graduates chose to marry, and the majority married within five years of their graduation. Unlike men, many women in our society can choose whether or not to undertake paid employment once they are married, and society counts it no stigma if they choose to stop work, particularly while their children are young. Women graduates therefore are often ambivalent about committing themselves to any career, and the majority of women who have obtained bachelor's degrees have never aspired to professional careers. Those who have done so have been particularly ambitious, choosing to combine the role of wife-mother-homemaker with that of a professional. (4) One can, therefore, look at the years since 1900 as ones in which women have striven, in a variety of ways, to resolve a marriage-career conflict; women aspiring to careers have concluded that they, like men, must be able to express themselves sexually and intellectually with equal freedom.

The century opened with a backlash to the advances in higher education made by women twenty years earlier. As more women applied for a university education, men faculty, students, and potential students began to feel threatened. By 1900 women made up over 50% of the Oberlin student body. At Michigan women were 22% of the total student body, and 47% of the literary department. Other universities had similar percentages. Some people claimed that men were being "driven out" of subjects like English, and were being denied admission because there was no room for them. Such claims were particularly serious when one considered how impotent women were as alumnae: a Minnie or a Nellie was unlikely to become a millionaire benefactor, but "Johns and Thomases have a habit of subscribing to gymnasium funds, and of voting legislative appropriations." (5) External pressures added to the pressure from alumni, faculty, and students against increasing the percentage of women students. At a time when the hereditary component in intelligence was believed to be paramount, it was thoroughly alarming to realize that the most intelligent women in the country were producing few children. Alarming also was the fact that the intelligent women most likely to attend college were of Anglo-Saxon stock, who were being outnumbered by new immigrants from southeastern Europe.

Mounting nativist feeling made it seem unwise to encourage girls to endanger their true avocation of motherhood by attending college, particularly when the psychologist, G. Stanley Hall, President of Clark University, reviving arguments which had originated many years earlier in less sophisticated form, claimed that too much brain-work was the greatest danger to women. Another pressure external to the universities, but affecting them nonetheless, was the crusade for moral purity. We think of

co-educational housing as a development of the late 1960s, but in the years before 1900 students, at some universities, lived in mixed rooming houses with no parietal regulations. Such arrangements began to seem undesirable as more women became students.

Universities like the University of Michigan responded to the pressures for moral reform by drawing up comprehensive regulations for the behavior of women students and by appointing a Dean of Women. They responded to the pressure against women students per se, by providing separate classes for men and women, and introducing quotas for the admission of women students. These restrictions lasted many decades, and in some cases quotas remain to this day, sometimes as unwritten but well-understood barriers to the admission of women. Despite restrictive measures, however, the number of women taking degrees increased slowly. By 1910 the percentage of first degrees awarded to women reached approximately 23%, the same percentage as were awarded in 1950. What happened during the forty years between those dates is an intriguing story of the interplay of personal desires and economic constraints. The graph that follows (Chart 1) shows how the percentage of women graduates rose steadily after 1910 to reach a peak in 1940, when women received 41% of all first degrees, and how the percentage fell precipitately after World War II. Only today, nearly thirty years later, has the percentage approached the 1940 figures again. The percentage of women earning higher degrees follows a slightly different pattern: 1930 was the decennial peak, never to be reached again so far at doctoral level.

By 1930, women made up 45% of professional and semi-professional workers, but with the Great Depression the economy contracted and women, particularly married ones, were discouraged from "taking jobs away from men." The percentage of first degrees earned by women continued to increase until the decade of the 1940s because a college education provided better job opportunities for young unmarried women, as well as the opportunity for meeting a husband whose potential for success was high. The steady decline in the percentage of master's and doctoral degrees earned by women reflected the fact that job opportunities for women did not increase at professional levels as they did for men, and that many women, since they received lower wages, had greater difficulty than men in financing graduate study. Women were particularly vulnerable to economic arguments against married women working, because societal disapproval of combining marriage and career was strong. Some of those who disapproved invoked Freud's ideas to support their position, while others, in popular magazines on child care, encouraged women to follow behaviorist techniques in child-rearing and to take the role of mother more seriously than ever before. (6)

The most serious set-back in the advance of higher education for women came, however, not with the Great Depression, but with the end of the Second World War, when the percentage of all degrees awarded to women dropped dramatically. The figures have to be studied with special care, because after World War II there was not a drop in the number of women taking degrees--on the contrary, at all levels, women earned more degrees than ever before. Rather, a huge increase in the number of men awarded degrees led to a change in the ratio of degrees awarded to men and women.

Chart 1-A. % distribution of bachelor's and first professional degrees earned by women, 1900-1970

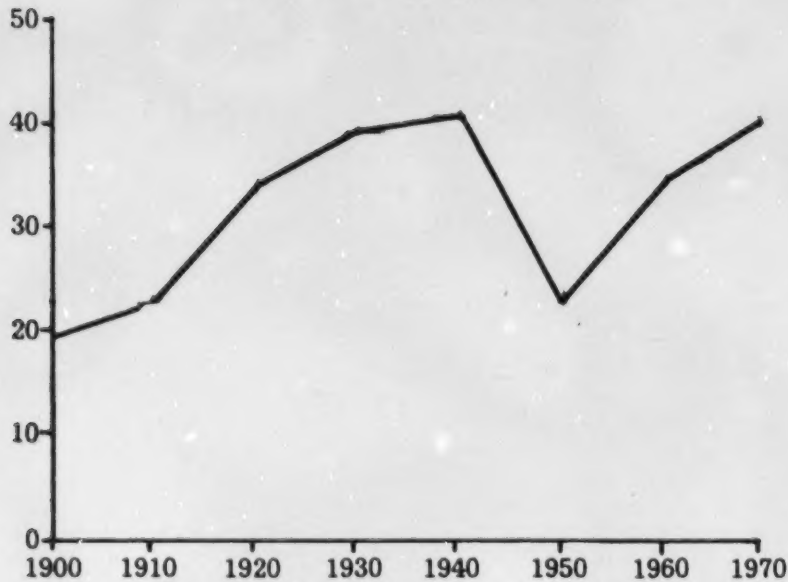
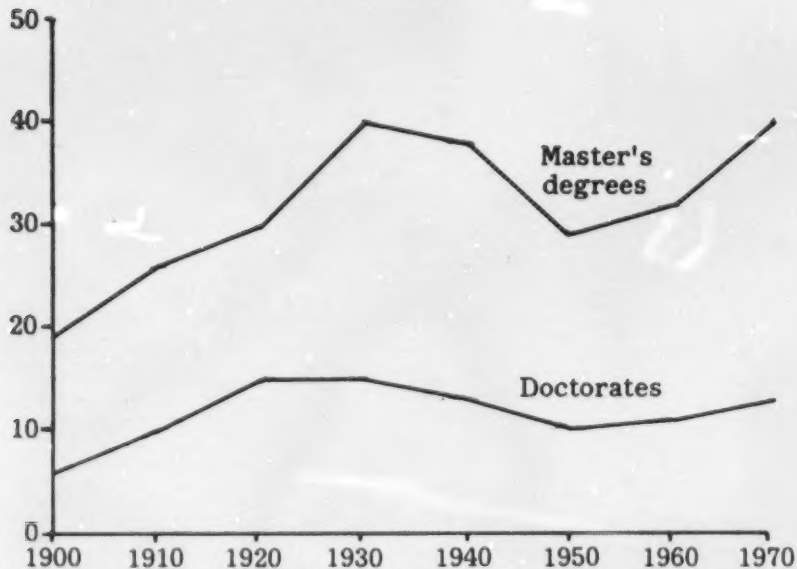


Chart 1-B: % distribution of second- and third-level degrees earned by women, 1900-1970



Source: Trends in Educational Attainment of Women, (U.S. Women's Bureau, Washington, D.C., 1968), 15. 1970 figures from Scientific Engineering Technical Manpower Comments, Vol. 9 (October 1974): 11.

Chart 2. Earned Degrees Conferred, by Level of Degree and Sex, Selected Years

Level of degree and sex	1940	1950
Total degrees	<u>216,521</u>	<u>498,373</u>
Women	87,606	121,513
Men	128,915	376,860
Bachelor's and first professional degrees	<u>186,500</u>	<u>433,734</u>
Women	76,954	103,915
Men	109,546	329,819
Master's and other second-level degrees	<u>26,713</u>	<u>58,219</u>
Women	10,223	16,982
Men	16,508	41,237
Doctor's and equivalent degrees	<u>3,290</u>	<u>6,420</u>
Women	429	616
Men	2,861	5,804
Percentage of total degrees		
Women	41%	24%
Men	59%	76%

Source: U.S. Department of Labor, Trends in Educational Attainment of Women (Washington, April 1968), 14.

Because the number of women awarded degrees has increased continually throughout the century, one has to look with suspicion at earlier studies of the post-World War II "return-to-the-home syndrome" which, based on case histories, have tended to regard it as the response of isolated individuals to amorphous societal pressures. Women in the post-war period apparently felt an urge to return to the home. Somehow, somewhere, they had lost the motivation that had driven their mothers to fight for the right to vote. If they went to college it was to broaden their horizons and to find suitable husbands. After college they might work for a short while, to help their husbands through graduate or professional school, but then they would stop work and raise a family. The data show that this was indeed the pattern adopted by many college women after the second world war. But this pattern was different from that followed by the majority of college educated women before the war only insofar as students after World War II married younger, and, therefore, it was married rather than unmarried young women graduates who entered the labor force.

That approximately 17,600 women received master's degrees and doctorates in 1950, against 10,700 in 1940 signifies an interest among a growing number of women in continuing their education, presumably with a view to professional careers. However, one phenomenon of the post-war period was a huge intake of veterans into the universities. Men, who might otherwise have taken jobs, took advantage of the G.I. bill to work for a degree. At the same time, money became available, particularly in the sciences, from several sources for the development of university graduate programs, and job specifications of professional and semi-professional work came increasingly to include an advanced degree. This change of job specification greatly affected women and was partly responsible for the drop in the percentage of women in professional and semi-professional occupations.

Women college graduates after the war tended to marry younger, so that the married couple became the unit that had to finance graduate study. In these circumstances it was unusual for the couple to have both partners in graduate school; usually the wife found a job while the husband continued his education. Since men could earn higher salaries than women as professionals, this strategy maximized potential income. Many women found clerical work to be most appropriate in the short term: training was brief, starting salaries relatively high, jobs easy to get anywhere, and the work compatible with running a house. Some found that being a secretary to professionals was a substitute for becoming professionals themselves.

The woman who aspired to become a professional faced enormous handicaps. After 1940 the sciences and technical fields at the universities expanded most. Traditionally they had attracted few women, and after the war little effort was made to encourage women to consider careers in new scientific or technical fields. At Carnegie Institute of Technology women received only 36 out of 1498 master's degrees in Engineering and Science awarded between 1946 and 1962. (7) Some university departments deliberately discouraged women graduate students because faculty felt they

were unlikely to "finish the course," or, as had been argued earlier in the century with regard to bachelor's candidates, they would be taking the place of men whose career commitments were more reliable.

The reluctance of many women even to consider a professional career stemmed not only from the difficulties of obtaining a second or third degree, but from the difficulties of advancing professionally once qualified. At a time when men graduates outnumbered women to an extent unknown for more than forty years, women's chances of being chosen for jobs or for promotion were small. Had many women of the 1945-1955 college graduating classes progressed regularly up the career ladder, they would now be among the managers, officials, and senior professors. However, as Weiss, Harwood, and Riesman point out with regard to employment figures for 1968: "Though women are over-represented in white-collar jobs, they are more apt to be office workers than managers and officials. . . . In short, though women are spared the heaviest manual labor, they tend to be order-takers rather than order-givers in the economy." (8)

That women's aspirations had been thwarted by the post-war surge of education for men became evident when the college generation of 1945-1950 reached their thirties. Then, women who had never gone to college or had withdrawn from it wanted to complete their college education and prepare for careers. In 1960 the University of Minnesota organized a counseling center to guide these returnees. This was the first of a series of continuing education programs for mature women at universities across the country. (9) At Radcliffe College, President Mary Bunting initiated an institute for women already professionally trained, in the belief that "women whose family responsibilities had prevented them from conforming to conventional employment patterns would. . . perform more effectively if offered flexible schedules, institutional affiliation, wider professional contacts." (10) With institutional assistance many women re-entered the labor market at professional or semi-professional levels but, even so, they remained professionally retarded when compared to their men colleagues.

To maintain or to increase women's representation among the order-givers in society, a large number of men and women will have to restructure their lives and reassess the relative importance of a career to each partner in a couple. Up to now most couples have based their status on the achievement of the man. As a result, many women have used academic attainment solely to acquire the most eligible husbands. Today, women increasingly look to themselves and their own achievements, rather than to their husbands', for their definition of status. The total number of degrees awarded to women in 1970 shows a substantial increase over the 1966 figure: 433,594 to 274,197; while the percentage of degrees awarded to women at bachelor's level has just exceeded the pre-war peak, and the percentage of advanced degrees is once again approaching it. (11) The figures, however, tell us nothing about attitudinal changes among the men and women that are accompanying this expansion. Alternative lifestyles among today's students may presage a change in the structure of administrative and executive jobs in the next ten years. Many men as well as women are no longer prepared to subordinate their whole beings to the demands of their jobs. As Ginzberg wrote in the mid-sixties: "It is no

longer simply that more and more women are following the pattern usually followed by men; men are also beginning to enjoy the broadened options that have become available to many women. The place of work in the lives of women is being radically altered, but this change has its counterpart in the lives of men. We are in the midst of a larger revolution." (12)

Footnotes

1. Professional, technical, and kindred occupations: percent distribution of women.

1930	45%	Note: Any increase or decline in percentage of degrees awarded to women is reflected in employment patterns at a later date (compare these percentages with Chart 1). As with percent distribution of degrees awarded, these percentages conceal a continuous increase in the actual numbers of women in this category.
1940	44%	
1950	40%	
1960	36%	
1970	39%	

Source: Statistical Abstract of the U.S. 1972 (Bureau of the Census, Washington, D.C., 1972), 230.

2. Bruno Bettelheim, "The commitment required of a woman entering a scientific profession in present-day American society," Jacquelyn A. Mattfeld and Carol G. Van Aken, eds., Women and the Scientific Professions (Cambridge, 1965), 3-19.
3. Figures quoted here and elsewhere in this paper, unless otherwise noted, have been taken from: Historical Statistics of the United States: Colonial Times to 1957 (Bureau of the Census, Washington, D.C., 1960), 212; Trends in Educational Attainment of Women (U.S. Women's Bureau, Washington, D.C., 1968).
4. See: Elizabeth M. Almquist and Shirley S. Angrist, "Role model influences on college women's career aspirations," Athena Theodore, ed., The Professional Woman (Cambridge, 1971), 301.
5. Albert Jacobs, as quoted by Dorothy G. McGuigan, A Dangerous Experiment (Ann Arbor, 1970), 100. McGuigan gives a vivid account of the reaction to co-education in chapter 13.
6. For a discussion of the popular literature on child-care see: A. Michael Sulman, The Freudianization of the American Child: The Impact of Psychoanalysis in Popular Periodical Literature in the United States, 1919-1938 (Ph.D. dissertation, University of Pittsburgh, 1972; Ann Arbor, University Microfilms, 1972).
7. Figures quoted are from: Joan N. Burstyn, "Educational experiences for women at CMU: a background," Final Report of the Commission on the Status and Needs of Women at Carnegie-Mellon University (Pittsburgh, 1971), 22.

8. Robert S. Weiss, Edwin Harwood, and David Riesman, "Work and automation: problems and prospects," Robert K. Merton and Robert Nisbet, Contemporary Social Problems, 3rd edition (New York, 1971), 552-553.
9. For details of continuing education programs see: Continuing Education Programs and Services for Women (U.S. Women's Bureau, Washington, D.C., 1968).
10. Alice K. Smith, Radcliffe Institute 1960 to 1971 (Cambridge, 1972), 1.
11. Figures for 1970 have been taken from: Scientific, Engineering Technical Manpower Comments, vol. 9, no. 9 (October 1972), 11.
12. Eli Ginzberg, Educated American Women: Life Styles and Self-portraits (New York, 1966), 178.

Overcoming Contemporary Barriers

Currently, studies are being made to determine what could be done right now to enable more women talented in science to disregard inappropriate societal pressures and pursue scientific careers. In the following excerpt, some of the more interesting findings to date are outlined.

WOMEN IN SCIENCE: OVERCOMING BARRIERS TO PARTICIPATION

Excerpted from "Women in Science," Mosaic (National Science Foundation) 6 (November-December 1975): 8-13.

Studies of successful women in science reveal that they have come from a very thin slice of society. Most were from white, middle or upper class families in which both parents were educated and able to help finance college educations. Families were small, and relationships warm and supportive. The father's role was especially important. He treated his daughter as an interesting person. He encouraged her development and the idea that her development didn't jeopardize her femininity. Thus, when she started school and encountered sexual stereotypes, she was prepared to deal with them because of the support she received at home.

The continuing presence of a supportive environment has been found to be essential to the development of the woman scientist. At a Conference on Successful Women in the Sciences, convened by the New York Academy of Sciences in 1972, Barbara Miller Solomon, assistant dean of Harvard College, summarized the need this way: "Women's expectations have grown because some people and some institutions have believed in them. Whether during childhood, school days, college, graduate, or professional years, these women have had special encouragement from one or more people: father, mother, teacher, friend, or professional colleague."

The high school years mark the educational beginnings of career choices, particularly in the case of science. Because learning in science is cumulative, potential scientists must enroll in the right courses. (Mathematics has proved a special problem, and many college freshman women find themselves lacking the prerequisite math for a science major.) Still, the proportion of women entering college who intend to major in science has increased slowly, climbing from 22.3 percent in 1969 to 26.5 percent in 1974, according to the American Council on Education. (In contrast, the number of women entering law and medicine has increased dramatically.)

The pressures on the would-be woman scientist are particularly intense in a co-educational school, a fact that no one, according to Dorothy Zinberg of Radcliffe, hides from her for a moment--"from her freshman adviser who insists mathematics will be too difficult or her classmates who assure her that she will give up science as most girls do, or her professors who, for the most part, do not take her quite as seriously as her male counterparts."

Perhaps for this reason, women's colleges appear to have been more productive of women with successful careers, according to data developed by M. Elizabeth Tidball, professor of physiology at the George Washington University Medical Center. Analyzing 1,500 subjects listed in three editions of "Who's Who of American Women," she found that graduates of women's colleges were more than twice as likely to have been cited for career achievement as were women graduates of coeducational colleges. The higher the percentage of men students, the smaller the output of women achievers.

Conversely, as the ratio of women faculty to women students increased, so did the output of women achievers. The correlation between the number of women faculty and number of career achievers was highly significant. . . . In another study, Tidball finds that among women who have received doctorates in biological and physical sciences since 1920, women's colleges have contributed almost a third, even though they have granted less than 15 percent of the bachelor's degrees in those subjects in the same time span.

Tidball notes that the number of women faculty in all undergraduate institutions has been declining for the past 40 years, while the number of women students has accelerated markedly during the past decade, especially in the coeducational colleges. Thus, she fears that the development of talent in young women will lag unless these trends are reversed. Many colleges where the most beneficial conditions exist--high women-faculty/women-student ratios and no men students--are closing for lack of financial support, while men's colleges have begun to compete for women students.

The following contemporary article illustrates perhaps the most devastating kind of response women have received when they try to enter male-dominated educational strongholds: a complete denial of their ability. We have seen how attempting to study physiology in the medical schools of Germany resulted in many problems for Ida Hyde, but she had the support of faculty members and students (see Walsh article on support groups). Also the criticism she faced was overt, and therefore perhaps easier to endure than the more subtle and devastating type of discrimination experienced by Evelyn Fox Keller.

Dr. Keller's experiences in trying to enter the all-male field of theoretical physics at Harvard show us that tactics similar to those used to block women's educational progress at the turn of the century were still around in 1957, and traces may still remain in the educational systems of today. This article is important not because it documents prejudice, but because of the author's realization years later that what she had perceived as personal inadequacies were actually external factors coming from her environment.

THE ANOMALY OF A WOMAN IN PHYSICS

by Evelyn Fox Keller

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A couple of months ago I was invited to give a series of lectures at a major university as one of a "series of distinguished guest lecturers" on mathematical aspects of biology. Having just finished teaching a course on women at my own college, I somehow felt obliged to violate the implicit protocol and address the anomalous fact of my being an apparently successful woman scientist. Though I had experienced similar vague impulses before, for a variety of reasons arising from a mix of anger, confusion, and timidity, it had never seemed to me either appropriate or possible to yield to such an impulse. Now, however, it seemed decidedly inappropriate, somewhat dishonest, and perhaps even politically unconscionable to deliver five lectures on my work without once making reference to the multitude of contradictions and conflicts I had experienced in arriving at the professional position presumed on this occasion. Therefore, in a gesture that felt wonderfully bold and unprofessional, I devoted the last lecture to a discussion of the various reasons for the relative absence of women in science, particularly in the higher ranks. The talk formed itself--with an ease, clarity, and lack of rancor that amazed me. I felt an enormous sense of personal triumph. Somehow, in the transformation of what had always appeared to me an essentially personal problem into a political problem, my anger had become depersonalized, even defused, and

a remarkable sense of clarity emerged. It suggested to me that I might, now, be able to write about my own rather painful and chaotic history as a woman in science.

Origins are difficult to determine and obscure in their relation to final consequences. Suffice it to say that in my senior year of college I decided I would be a scientist. After several years of essentially undirected intellectual ambition, I majored in physics partly for the sake of discipline and partly out of the absence of any clear sense of vocation; and in my last year I fell in love with theoretical physics.

I invoke the romantic image not as a metaphor, but as an authentic, literal description of my experience. I fell in love, simultaneously and inextricably, with my professors, with a discipline of pure, precise, definitive thought, and with what I conceived of as its ambitions. I fell in love with the life of the mind. I also fell in love, I might add, with the image of myself striving and succeeding in an area where women had rarely ventured. It was a heady experience. In my adviser's fantasies, I was to rise, unhampered, right to the top. In my private fantasies, I was to be heralded all the way.

It was 1957. Politics conspired with our fantasies. Graduate schools, newly wealthy with National Science Foundation money, competed vigorously for promising students, and a promising female student was a phenomenon sufficiently unique to engage the interest and curiosity of recruiters from Stanford to Harvard. Only Cal Tech and Princeton were closed to me--they were not yet admitting women--and I felt buoyant enough to challenge them. I particularly wanted to go to Cal Tech to study with Richard Feynman--a guru of theoretical physics--on whose work I had done my senior thesis. In lieu of my being accepted at Cal Tech, an influential friend of mine volunteered to offer Feynman a university chair at MIT, where I would be admitted. Heady indeed.

Even then I was aware that the extreme intoxication of that time was transitory--that it had primarily to do with feeling "on the brink." Everything that excited me lay ahead. I had fantasies of graduate school and becoming a physicist; what awaited me, I thought, was the fulfillment of those fantasies. Even the idea of "doing physics" was fantasylike. I could form no clear picture of myself in that role, had no clear idea of what it involved. My conception of a community of scholars had the airiness of a dream. I was intoxicated by a vision that existed primarily in my head.

Well, Feynman was not interested in leaving Cal Tech, and so I went to Harvard. More accurately, I was pressured, and eventually persuaded, by both a would-be mentor at Harvard and my adviser, to go to Harvard. At Harvard I was promised the moon and the sun--I could do anything I wanted. Why I was given this extraordinary sales pitch seems, in retrospect, all but inexplicable. At the time, it seemed quite natural. I dwell on the headiness of this period in order to convey the severity of the blow that graduate school at Harvard actually was.

The story of my graduate school experience is a difficult one to tell. It is difficult in part because it is a story of behavior so crude and so extreme as to seem implausible.

Moreover, it is difficult to tell because it is painful. In the past, the telling of this story always left me so badly shaken, feeling so exposed, that I became reluctant to tell it. Many years have passed, and I might well bury those painful recollections. I do not because they represent a piece of reality--an ongoing reality that affects others, particularly women. Even though my experiences may have been unique--no one else will share exactly these experiences--the motives underlying the behavior I am going to describe are, I believe, much more prevalent than one might think, and detectable in fact in behavior much less extreme.

I tell the story now, therefore, because it may somehow be useful to others. I can tell the story now because it no longer leaves me feeling quite so exposed. Let me try to explain this sense of exposure.

Once, several months into my first year in graduate school, a post-doctoral student in an unusual gesture of friendliness offered me a ride home from a seminar and asked how I was doing. Moved by his gesture, I started to tell him. As I verged on tears, I noticed the look of acute discomfort on his face. Somehow, I had committed a serious indiscretion. It was as if I had publicly disrobed. Whatever I said, then and always after, it somehow seemed I had said too much. Some of this feeling remains with me even now as I write this article. It is a consequence of the assumption in the minds of others that what I am describing must have been a very personal, private experience--that is, that it was produced somehow by forces within myself. It was not. Although I clearly participated in and necessarily contributed to these events, they were essentially external in origin. That vital recognition has taken a long time. With it, my shame began to dissolve, to be replaced by a sense of personal rage and, finally, a transformation of that rage into something less personal--something akin to a political conscience.

That transformation, crucial in permitting me to write this, has not, however, entirely removed the pain from the process of recollecting a story that retains for me considerable horror. If I falter at this point, it is because I realize that in order for this story to be meaningful, even credible, to others, I must tell it objectively--I must somehow remove myself from the pain of which I write. The actual events were complex. Many strands weave in and out. I will describe them, one by one, as simply and as fairly as I can.

My first day at Harvard I was informed, by the very man who had urged me to come, that my expectations were unrealistic. For example, I could not take the course with Schwinger (Harvard's answer to Feynman) that lured me to Harvard, and I ought not concern myself with the foundations of quantum mechanics (the only thing that did concern me) because, very simply, I was not, could not be, good enough. Surely my ambition was based on delusion--it referred to a pinnacle only the very few, and certainly not I, could achieve. Brandeis, I was told bluntly, was not

Harvard, and although my training there might have earned me a place at Harvard, distinction at Brandeis had no meaning here. Both I and they had better assume I knew nothing. Hence I ought to start at the beginning. The students they really worried about, I was informed, were those who were so ignorant and naive that they could not apprehend the supreme difficulty of success at Harvard.

These remarks were notable for their blatant class bias and arrogance, as well as for their insistent definition of me on the basis of that bias--a gratuitous dismissal of my own account that I experienced recurrently throughout graduate school. The professor's remarks were all the more remarkable in that I had expressed exactly the same intentions in our conversation the previous spring and had then been encouraged. What could account for this extraordinary reversal? There had been no intervening assessment of my qualifications. Perhaps it can be explained simply by the fact that the earlier response was one of someone in the position of selling Harvard, while now it seemed there was an obligation to defend her. (It is ironic that universities should be associated with the feminine gender.) Nor was it coincidental, I suspect, that this man was shortly to assign to one of the senior graduate students (male, of course) the task of teaching me how to dress. (1)

Thus began two years of almost unmitigated provocation, insult, and denial. Lacking any adequate framework--political or psychological--for comprehending what was happening to me, I could only respond with personal rage: I felt increasingly provoked, insulted, and denied. Where political rage would have been constructive, personal rage served only to increase my vulnerability. Having come to Harvard expecting to be petted and fussed over (as I had been before) and expecting, most of all, validation and approval, I was entirely unprepared for the treatment I received. I could neither account for nor respond appropriately to the enormous discrepancy between what I expected and what I found. I had so successfully internalized the cultural identification between male and intellect that I was totally dependent on my (male) teachers for affirmation--a dependency made treacherous by the chronic confusion of sexuality and intellect in relationships between male teachers and female students. In seeking intellectual affirmation, I sought male affirmation, and thereby became exquisitely vulnerable to the male aggression surrounding me.

I had in fact been warned about the extreme alienation of the first year as a graduate student at Harvard, but both my vanity and my naivete, permitted me to ignore these warnings. I was confident that things would be different for me. That confidence did not last long. Coming from everywhere, from students and faculty alike, were three messages. First, physics at Harvard was the most difficult enterprise in the world; second, I could not possibly understand the things I thought I understood; and third, my lack of fear was proof of my ignorance. At first, I adopted a wait-and-see attitude and agreed to take the conventional curriculum, though I privately resolved to audit Schwinger's course. Doing so, as it turned out, seemed such an act of bravado that, daily, all eyes turned on me as I entered the class and, daily, I was asked by half a dozen people with amusement if I still thought I understood. Mysteriously,

my regular courses seemed manageable, even easy, and as I became increasingly nervous about my failure to fear properly, I spent more and more evenings at the movies. In time, the frequent and widespread iteration of the message that I could not understand what I thought I understood began to take its toll. As part of a general retreat, I stopped attending Schwinger's course. I had begun to lose all sense of what I did or did not understand, there and elsewhere. That I did well in my exams at the end of the semester seemed to make no difference whatever.

Meanwhile, it was clear that I was becoming the subject--or object--of a good deal of attention in the Physics Department. My seriousness, intensity, and ambition seemed to cause my elders considerable amusement, and a certain amount of curiosity as well. I was watched constantly, and occasionally addressed. Sometimes I was queried about my peculiar ambition to be a theoretical physicist--didn't I know that no woman at Harvard had ever so succeeded (at least not in becoming a pure theoretical physicist)? When would I too despair, fail, or go elsewhere (the equivalent of failing)? The possibility that I might succeed seemed to be a source of titillation; I was leered at by some, invited now and then to a faculty party by others. The open and unbelievably rude laughter with which I was often received at such events was only one of many indications that I was on display--for purposes I could either not perceive or not believe. My fantasy was turning into nightmare.

In lieu of support I began to long for anonymity, but the anomaly of my position had made it so public that there was no hiding. My real world began to resemble a paranoid delusion. Many people in Cambridge knew who I was and speculated about me. None of them offered friendship. Once, feeling particularly lonely on a Saturday night, I went for a walk by the Charles River. I was sitting on a bench, deep in thought, when a young man, a stranger, appeared out of the dark, sat down, and began to recite a detailed knowledge of me--what I was doing, who my friends were, where I had come from, and other particulars. Finishing his recitation, he got up and walked away. When real events so take on the qualities of delusion, it becomes difficult indeed to expect the credulity of others. Yet I remained frighteningly sane and had no difficulty in recognizing the reality--only in understanding it.

It is sometimes hard to separate affront to oneself as a person from affront to one's sensibilities. Not only do they tend to generate the same response--one feels simply affronted--but it is also possible (as I believe was true here) that the motives for both affronts are not unrelated. I went to graduate school with a vision of theoretical physics as a vehicle for the deepest inquiry into nature--a vision perhaps best personified, in recent times, by Einstein. The use of mathematics to further one's understanding of the nature of space, time, and matter represented a pinnacle of human endeavor. I went to graduate school to learn about foundations. I was taught, instead, how to do physics. In place of wisdom, I was offered skills. Furthermore, this substitution was made with moralistic fervor. It was wrong, foolhardy, indeed foolish, to squander precious time asking why. Proper humility was to bend to the grindstone and learn techniques. Contemporary physics, under the sway of

operationalism, had, it seemed, dispensed with the tradition of Einstein--almost, indeed, with Einstein himself. General relativity, the most intellectually ambitious venture of the century, seemed then (wrongly) a dead subject. Philosophical considerations of any sort in the physical sciences were at an all-time low. Instead, techniques designed to calculate nth-order corrections to a theory grievously flawed at its base were the order of the day.

Physics had become a major industry. Huge investments poured into experiments, the results of which needed subsequently to be matched by theoretical calculations. Paralleling the influx of money was an influx of manpower. They couldn't all be creative innovators. The Baconian vision of an army of scientific foot soldiers was imminent; and Harvard physicists, whatever they were doing, were, by definition, the best--they were generals. The status of the elite had to be protected even though the very conception of elite was uncertain. While there was general agreement about what the student rank and file should (or rather, should not) be doing, the generals seemed considerably confused about what they should be doing. The work of Harvard's most distinguished theoretician conveyed a sense of grand sweep. While he was roundly faulted and criticized for the abstract formality of his approach, his status as the best went unchallenged. Even among the elite, then, there was a lack of clarity about the nature and rules of progress and excellence, and a certain amount of scrambling. Somehow the notion of different but equally valid postures did not seem tenable--the preoccupation with ranking was overriding.

My naivete, and idealism were perfect targets. Not only did I not know my place in the scheme of things as a woman, but by a curious coincidence, I was apparently equally ingenuous concerning my place as a thinker. I needed to be humbled. Though I writhed over the banality of the assignments I was given, I did them, acknowledging that I needed in any case to learn the skills. I made frequent arithmetic errors--reflecting a tension that endures within me even today between the expansiveness of conception and the precision of execution, my personal variation perhaps of the more general polar tension in physics as a whole. When my papers were returned with the accuracy of the conception ignored and the arithmetic errors streaked with red--as if with a vengeance--I wondered whether I was studying physics or plumbing. Who has not experienced such a wrenching conflict between idealism and reality? Yet my fellow students seemed oddly untroubled. From the nature of their responses when I tried to press them for deeper understanding of the subject, I thought perhaps I had come from Mars. Why, they wondered, did I want to know? That they were evidently content with the operational success of the formulas mystified me. Even more mystifying was the absence of any appearance of the humility of demeanor that one would expect to accompany the acceptance of more limited goals. I didn't fully understand then that, in addition to the techniques of arrogance, this peculiar inversion in the meaning of humility was simply part of the process of learning how to be a physicist. It was intrinsic to the professionalization, and what I might even call the masculinization, of an intellectual discipline.

To some extent the things I describe here are in the nature of the academic subculture. They reflect the perversion of academic style--familiar in universities everywhere--a perversion that has become more extensive as graduate schools have tended to become increasingly preoccupied with professional training. My experiences resemble those of many graduate students--male and female alike. What I experienced as a rather brutal assault on my intellectual interests and abilities was I think no accident, but rather the inevitable result of the pervasive attempt of a profession to make itself more powerful by weeding out those sensibilities, emotional and intellectual, that it considers inappropriate. Not unrelated is a similar attempt to maintain the standards and image of a discipline by discouraging the participation of women--a strategy experienced and recounted by many other women. Viewed in this way, it is perhaps not surprising that the assault would be most blatant in a subject as successful as contemporary physics, and in a school as prestigious as Harvard.

Perhaps the most curious, undoubtedly the most painful, part of my experience was the total isolation in which I found myself. In retrospect, I am certain that there must have been like-minded souls somewhere who shared at least some of my disappointments. But if there were, I did not know them. In part, I attribute this to the general atmosphere of fear that permeated the graduate student body. One did not voice misgivings because they were invariably interpreted to mean that one must not be doing well. (2) The primary goal was to survive, and, better yet, to appear to be surviving, even prospering. So few complaints were heard from anyone. Furthermore, determined not to expose the slightest shred of ignorance, few students were willing to discuss their work with any but (possibly) their closest friends. I was, clearly, a serious threat to my fellow students' conception of physics as not only a male stronghold but a male retreat, and so I was least likely to be sought out as a colleague. I must admit that my own arrogance and ambition did little to allay their anxieties or temper their resistances. To make matters even worse, I shared with my fellow classmates the idea that a social or sexual relationship could only exist between male and female students if the man was "better" or "smarter" than the woman--or at the very least, comparable. Since both my self-definition and my performance labeled me as a superior student, the field of sociability and companionship was considerably narrowed.

There was one quite small group of students whom I did view as like-minded and longed to be part of. They too were concerned with foundations; they too wanted to know why. One of them (the only one in my class) had in fact become a close friend during my first semester. Though he preached to me about the necessity of humility, the importance of learning through the tips of one's fingers, the virtue of precision--he also listened with some sympathy. Formerly a Harvard undergraduate, he explained to me the workings of Harvard and I explained to him how to do the problems. With his assistance, I acquired the patience to carry out the calculations. We worked together, talked together, frequently ate together. Unfortunately, as the relationship threatened to become more intimate, it also became more difficult--in ways that are all too familiar--until, finally, he decided that he could no longer afford the risk of a close association

with me. Out of sympathy for his feelings, I respected his request that I steer clear of him and his friends--with the consequence that I was, thereafter, totally alone. The extent of my isolation was almost as difficult for me to believe as for those to whom I've attempted to describe it since. Only once, years later in a conversation with another woman physicist, did I find any recognition. She called it the "sea of seats": you walk into a classroom early, and the classroom fills up, leaving a sea of empty seats around you.

Were there no other women students? There were two, who shared neither my ambition, my conception of physics, nor my interests. For these reasons, I am ashamed to say, I had no interest in them. I am even more ashamed to admit that out of my desire to be taken seriously as a physicist I was eager to avoid identification with other women students who I felt could not be taken seriously. Like most women with so-called male aspirations, I had very little sense of sisterhood.

Why did I stay? The Harvard Physics Department is not the world. Surely my tenacity appears as the least comprehensible component of my situation. At the very least, I had an extraordinary tolerance for pain. Indeed, one of my lifelong failings has been my inability to know when to give up. The very passion of my investment ruled out alternatives.

I had, however, made some effort to leave. At the very beginning, a deep sense of panic led me to ask to be taken back at Brandeis. Partly out of disbelief, partly out of the conviction that success at Harvard was an invaluable career asset, not to be abandoned, I was refused, and persuaded to continue. Although I had the vivid perception that rather than succeed I would be undone by Harvard, I submitted to the convention that others know better; I agreed to suspend judgment and to persevere through this stinging "initiation rite." In part, then, I believed that I was undergoing some sort of trial that would terminate when I had proven myself, certainly by the time I completed my orals. I need be stoic only for one year. Unfortunately, that hope turned out to be futile. The courses were not hard, never became hard in spite of the warnings, and I generally got A's. But so did many other students. Exams in fact were extremely easy.

When I turned in particularly good work, it was suspected, indeed sometimes assumed, that I had plagiarized it. On one such occasion, I had written a paper the thesis of which had provoked much argument and contention in the department. This I learned, by chance, several weeks after the debate was well underway. In an effort to resolve the paradox created by my results, I went to see the professor for whom I had written the paper. After an interesting discussion, which incidentally resolved the difficulty, I was asked, innocently and kindly, from what article(s) I had copied my argument.

The oral exams, which I had viewed as a forbidding milestone, proved to be a debacle. My committee chairman failed to appear. The result was that I was examined by an impromptu committee of experimentalists on mathematical physics. Months later, I was offered the following

explanation: "Oh, Evelyn, I guess I owe you an apology. You see, I had just taken two sleeping pills and overslept." The exam was at 2:00 p.m. Nevertheless, I passed. Finally, I could begin serious work. I chose as a thesis adviser the sanest and kindest member of the department. I knocked on his door daily for a month, only to be told to come back another time. Finally I gained admittance, to be advised that I'd better go home and learn to calculate.

My second year was even more harrowing than the first. I had few courses and a great deal of time that I could not use without guidance. I had no community of scholars. Completing the orals had not served in any way to alleviate my isolation. I was more alone than ever. The community outside the physics department, at least that part to which I had access, offered neither solace nor support. The late fifties were the peak of what might be called home-brewed psychoanalysis. I was unhappy, single, and stubbornly pursuing an obviously male discipline. What was wrong with me? In one way or another, this question was put to me at virtually every party I attended. I was becoming quite desperate with loneliness. And as I became increasingly lonely I am sure I became increasingly defensive, making it even more difficult for those who might have been sympathetic to me or my plight to approach me to commiserate. Such support might have made a big difference. As it was, I had neither colleagues nor lovers, and not very many friends. The few friends I did have viewed my situation as totally alien. They gave sympathy out of love, though without belief. And I wept because I had no friend whose ambition I could identify with. Was there no woman who was doing, had done, what I was trying to do? I knew of none. My position was becoming increasingly untenable.

Had I been married, would I have fared differently? I came to believe not only that marriage offered the only support possible, but even more, that my failure to marry was somehow the root of all my difficulties. But, it seemed, my career choice, and my attitude toward it, discouraged all suitors. Where was the way out? Ideally, I had thought they would come together--physics and love. But they seemed to cancel each other out. My most frequent fantasy was that I would return to the Physics Department one day, victoriously, a physicist with a baby in my arms. My impoverished imagination could conceive of no better vindication. Even my fantasies conformed to stereotype.

What had happened to my dreams? Although I had, shortly after my orals, arranged to transfer out of Harvard at the end of the second year, when that time came, my hopes and plans (not to speak of momentum) were too thoroughly shattered for me to consider going elsewhere. After two years of virtually continuous denial of my perception, my values, and my ambitions--an experience that might then have been described as brain-washing, and ought now be called schizophrenogenic--my demoralization was complete. Feeling pushed to the wall, I decided, midyear, to give up physics and return to a world I realized must still exist, somewhere outside. Though I remained officially enrolled in graduate school in order to collect my monthly stipend, my commitment to physics was over. Even before I stopped attending classes formally, this change of heart was visible enough to trigger a remarkable change in my fellow students, who

became, overnight, friendly and sympathetic. Clearly, I was no longer a threat. Ironically, at the end of my second year, during which I had done virtually no work and attended few classes, I still got A's. I include this as a comment on the grading system at Harvard.

I recognize that this account reads in so many ways like that of a bad marriage--the passionate intensity of the initial commitment, the fantasies on which such a commitment (in part) is based, the exclusivity of the attachment, the apparent disappearance of alternative options, the unwillingness and inability to let go, and finally, the inclination to blame oneself for all difficulties. Although I can now tell this story as a series of concrete, objective events that involved and affected me, at the time I eventually came to accept the prevalent view that what happened to me at Harvard simply manifested my own confusion, failure, neurosis--in short that I had somehow "made" it happen. The implications of such internalization were--as they always are--very serious.

Now I had to ask how I had "made" it happen--what in me required purging? It seemed that my very ambition and seriousness were at fault, and that these qualities--qualities I had always admired in others--had to be given up. Giving up physics, then, seemed to mean giving up parts of me so central to my sense of myself that a meaningful extrication was next to impossible. I stayed on at Harvard, allowing myself to be convinced once again that I must finish my degree, and sought a dissertation project outside the Physics Department.

After drifting for a year, I took advantage of an opportunity to do a thesis in molecular biology while still nominally remaining in the Physics Department. That this rather unusual course was permitted indicated at least a recognition, on the part of the then chairman, of some of the difficulties I faced in physics. Molecular biology was a field in which I could find respect, and even more important, congeniality. I completed my degree, came to New York to teach (physics!), married, bore children, and ultimately began to work in theoretical biology, where I could make use of my training and talents. This proved to be a rewarding professional area that sustained me for a number of critical years. If my work now begins to take me outside this professional sphere, into more political and philosophical concerns, this reflects the growing confidence and freedom I have felt in recent years.

Inner conflict, however, was not to disappear with a shift in scientific specialization. While it is true that I was never again to suffer the same acute--perhaps bizarre--discomfort that I did as a graduate student in physics, much of the underlying conflict was to surface in other forms as I assumed the more conventional roles of wife, mother, and teacher. The fundamental conflict--between my sense of myself as a woman and my identity as a scientist--could only be resolved by transcending all stereotypical definitions of self and success. This took a long time, a personal analysis, and the women's movement. It meant establishing a personal identity secure enough to allow me to begin to liberate myself from everyone's labels--including my own. The tension between "woman" and "scientist" is not now so much a source of personal struggle as a profound concern.

After many years, I have carved out a professional identity very different from the one I had originally envisioned, but one that I cherish dearly. It is, in many important ways, extraprofessional. It has led me to teach in a small liberal arts college that grants me the leeway to pursue my interests on my own terms and to combine the teaching I have come to love with those interests, and that respects me for doing so. It has meant acquiring the courage to seek both the motives and rewards for my intellectual efforts more within myself. Which is not to say that I no longer need affirmation from others; but I find that I am now willing to seek and accept support from different sources--from friends rather than from institutions, from a community defined by common interests rather than by status.

As I finished writing this essay, I came across an issue of the annals of the New York Academy of Sciences (March 15, 1973) devoted to "Successful Women in the Sciences." The volume included brief autobiographical accounts of a dozen or so women, two of whom were trained in physics and one in mathematics. Because material of this kind is almost nonexistent, these first-person reports are an important contribution "to the literature." I read them avidly. More than avidly, for the remarks of these women, in their directness and honesty, represent virtually the only instance of professional circumstances with which to compare my own experience.

It may be difficult for those removed from the mores of the scientific community to understand the enormous reticence with which anyone, especially a woman, would make public his or her personal impressions and experiences, particularly if they reflect negatively on the community. To do so is not only considered unprofessional, it jeopardizes one's professional image of disinterest and objectivity. Women, who must work so hard to establish that image, are not likely to take such risks. Furthermore, our membership in this community has inculcated in us the strict habit of minimizing any differences due to our sex. I wish therefore to congratulate women in the mainstreams of science who demonstrate such courage.

Their stories, however, are very different from mine. Although a few of these women describe discrete experiences similar to some of mine, they were generally able to transcend their isolation and discomfort, and in their perseverance and success, to vindicate their sex. I am in awe of such fortitude. In their stories I am confirmed in my sense that with more inner strength I would have responded very differently to the experiences I've recorded here. The difficulty, however, with success stories is that they tend to obscure the impact of oppression, while focusing on individual strengths. It used to be said by most of the successful women that women have no complaint precisely because it has been demonstrated that with sufficient determination, anything can be accomplished. If the women's movement has achieved anything, it has taught us the folly of such a view. If I was demolished by my graduate school experiences, it was primarily because I failed to define myself as a rebel against norms in which society has heavily invested. In the late fifties, "rebel" was not a meaningful word. Conflicts and obstacles were seen to be internal. My insistence on

maintaining a romantic image of myself in physics, on holding to the view that I would be rewarded and blessed for doing what others had failed to do, presupposed a sense of myself as special, and therefore left me particularly vulnerable. An awareness of the political and social realities might have saved me from persisting in a search for affirmation where it could not and would not be given. Such a political consciousness would have been a source of great strength. I hope that the political awareness generated by the women's movement can and will support young women who today attempt to challenge the dogma, still very much alive, that certain kinds of thought are the prerogative of men.

Footnotes

1. My attire, I should perhaps say, was respectable. It consisted of skirts and sweaters, selected casually, with what might have been called a bohemian edge. I wore little or no makeup.
2. Indeed, most people then and later assumed I had done badly--particularly after hearing my story. Any claims I made to the contrary met with disbelief.

Barriers to equal opportunity may arise because of sex, race, or ethnic identity. As a result, minority women may face a double barrier to their full participation in science careers. The sex- and race-related barriers encountered by minority women scientists in their education and work are discussed in the following article.

THE DOUBLE BIND:

THE PRICE OF BEING A MINORITY WOMAN IN SCIENCE

by Shirley Mahaley Malcolm, Paula Quick Hall, and Janet Welsh Brown

Condensed and adapted from a report (No. 76-R-3, April 1976) by Shirley Mahaley Malcom, Paula Quick Hall, and Janet Welsh Brown. American Association for the Advancement of Science, 1515 Massachusetts Avenue, NW, Washington, D.C. 20005.

Minority women represent a disturbingly small part of the total scientific workforce (Table 1). This situation has not been improved by past programs to provide equal opportunity, because programs developed for minorities in science are usually dominated by men, and programs designed for women are usually dominated by majority women. To devise new programs for minority women, The Office of Opportunities in Science of AAAS (American Association for the Advancement of Science) organized a meeting of minority women scientists. The thirty individuals participating in this meeting were drawn from a larger pool of almost 200 minority women scientists nominated by their colleagues. These Black, Mexican-American, Native American, and Puerto Rican women represented various fields of science, engineering, and medicine, as well as a variety of educational and work experiences, geographical areas, and generations. At the meeting, held in December 1975, the women analyzed their experiences and made recommendations for programs to increase their numbers and improve their status in science. Important points derived from their shared experiences are summarized in this report.

Minority women scientists perceive that their early experiences with racism are superseded by problems of sexism as they progress through graduate and professional education and the working world. Many incidents occur which could be covered by race bias or gender bias or both. When these situations arise, it becomes difficult if not impossible to determine which "ism" is in force. In such cases, it does not matter whether one is being hit with the club of sexism or racism--they both hurt. And this is the nature and the essence of the double bind.

The Precollegiate Experience

The formal education of the conferees began and remained on a plane that was negative-to-neutral toward science. Because of the different

Table 1. Minority Women in the 1970 Labor Force, by Field

	Black		Amer. Indian		Asian		Hispanic	
	Coll/U Teachers	All Other	Coll/U Teachers	All Other	Coll/U Teachers	All Other	Coll/U Teachers	All Other
Chemistry	54	181	-	-	74	938	22	275
Physics	-	91	-	-	-	19	19	19
Geology	-	17	-	-	25	-	-	-
Marine Sciences	-	-	-	-	25	-	-	-
Atmos./Space Sci.	-	55	-	-	25	20	-	-
Mathematics	244	885	44	42	112	284	34	245
Engineering	76	757	-	63	-	249	36	298
Ag. Sciences	15	17	-	-	43	-	-	20
Biol. Sciences	106	641	30	17	244	355	22	129
Psychology	129	583	-	-	105	152	-	133
Economics	15	484	-	-	40	20	-	187
Sociology	132	126	-	-	19	-	-	-
Total	825	3,837	74	172	761	2,037	136	1,206

Source: 1970 Census, Occupational Characteristics, June 1973.

types of schools attended by these minority women, there were qualitative differences in the nature of their precollegiate educational experiences.

Some of the conferees attended segregated schools and were taught by members of their own race or ethnic group. These women valued highly the positive influence of teachers who typically expected much of them, seemed to have a personal interest in them, and motivated and supported their intellectual curiosity. The segregated minority schools, however, usually suffered from inadequate facilities, although the students were not always aware of the differences until they had exposure to other schools.

Most of the women who attended predominately majority or integrated schools were challenged by a competitive atmosphere and high academic standards. Teachers, however, often had lower expectations of minority students. In some instances conflict among students of different racial or ethnic groups made school a tense environment, less conducive to study and normal social interaction. Social isolation and a dearth of same-sex, same-race peer companionship had a marked influence on the adolescent development of most conferees who attended this type of school.

Students attending schools designed for minority students but staffed by majority faculty frequently experienced a patronizing, missionary attitude on the part of teachers. Little was expected of them academically. Counseling, whether formal or unstructured, was toward the less

academically demanding courses and career choices. Insensitivity and ignorance of cultural value differences on the part of teachers was another barrier to student achievement.

When the women first went off to college, most had in mind careers in teaching at the primary or secondary level where they expected someone to share their interest in science. A few aspired to medicine. Except for the more recent graduates who went into engineering programs straight from high school, most of the scientists had known little about other careers in science; they had limited their own career aspirations to the traditional roles they knew best.

Collegiate and Professional Education

Most of the women scientists at the conference had experienced financial strain during college years. This strain was sometimes magnified by inadequate information about available financial aid.

For these women, course work at the undergraduate level was somewhat difficult, usually because secondary school had not been sufficiently demanding for them to have developed strict self-discipline and good study habits. If rigorous laboratory science had not been available prior to college, orientation to lab work was an additional hurdle. Most of these women scientists felt (as do most scientists), that undergraduate exposure to research would have been beneficial. Exclusion from informal study groups, usually all male and/or majority, made their pursuit of academic excellence difficult. Although most remembered at least one teacher who had encouraged, counseled, and assisted her through a trying time, many had encountered a patronizing attitude on the part of majority science professors. In this circumstance it was especially hard for the students to ask for routine help when they did need it.

When entering graduate school, the student usually faced a dramatic change in teaching approaches and emphasis. Typically undergraduate training in science was classical and conservative, with heavy supervision. Graduate school was more experimental and research-oriented, with very little supervision. For those who had attended the supportive women's or minority schools, adjustment to the different teaching styles, philosophies, standards, expectations, and a less emotionally supportive environment was especially difficult. Academic difficulties for these students were usually due to inadequacies of undergraduate courses and/or equipment. Lack of support from major advisors was among the most crucial difficulties during graduate work. Without this support it was often impossible to secure essential research grants, postdoctoral appointments, and employment.

Loneliness, pressure to choose a traditional career, to marry, to remain in or return to the community of their youth--in summary, to fill cultural role expectations--was constant. While this isolation existed at both the graduate and undergraduate levels, in the experience of most conferees it became progressively worse the farther they progressed in their academic work.

Career and Professional Experience

Despite the growth of anti-affirmative action sentiments, employers appear to have aggressively recruited the minority woman scientists and engineers. This phenomenon did not, however, always result in firm, appropriate offers of employment and indeed may have benefited only the most recent graduates. The participants noted differences in the salary/benefit packages traditionally offered men and women in academe, industry and, in the experience of one conferee, a hospital. Without having prior knowledge of the negotiability of terms and conditions of employment, or knowledge of what others were receiving or being offered, many of these scientists found themselves in a disadvantaged bargaining position. All agreed that majority males had a tremendous advantage in having access to an informal system of "contacts" for entry and advancement in the labor force.

The discussants identified several areas of concern in the work experience. In most of their experience up to college and graduate or professional school, racism and problems related to their ethnicity had been the basis of obstacles they faced. In the world of work sexism became more apparent. Despite their realization of this shift, most of the scientists identified more strongly with their racial or ethnic groups than with women, but noted that, in either case, they were being pressured to choose sides and to identify themselves as one or the other.

These scientists generally achieved a large measure of personal gratification from their work, despite a number of hardships. Most felt that they were functioning effectively in their jobs and expressed satisfaction with what they were doing. Frequently, this satisfaction resulted from a reordering of priorities. Through accommodation to circumstances that did not quite meet their needs and/or possession of values atypical of men in their profession, many of these women have maintained viable careers in science. In very few instances did they note efforts or actual accommodation on the part of the institutions that employed them.

Although the industrial work setting provided some of the most blatant examples of the indignities and repression experienced by these women, discrimination in academe was reported to be equally pervasive but more subtle and perhaps more damaging. Even when men and women began at the same level, unfair promotion policy and/or practice advanced the men at a much faster rate; if one began below the appropriate level there was practically no chance of ever "catching up."

Men were often presumed to have "innate" abilities and suitable personality traits for management level positions whereas minority women needed "credentials." Women were often excluded from the system of "grooming" a protégé, or providing informal on-the-job training. Many rewards were conferred and decisions made "over lunch" or "over cocktails" or at other social gatherings from which these women were excluded. The de facto or systematic exclusion of minority women from personnel, tenure, and other committees which decide promotions in some organizations reinforced existing discriminatory employment practices. The most distressing

aspect of this situation was that their absence seemed of little concern to the individuals in authority.

The conferees felt strongly that management and administrators consistently gave different interpretations to identical behavior by males and females. A woman might be considered "pushy" for doing what is assertive for a man, or "emotional and loquacious" when compared to a "sensitive, articulate" male. Most women at the conference had adapted to often uncomfortable personality styles in order to advance in their professions. In some cases the style of behavior required by their culture made this adaptation even more difficult than it is for middle-class white women.

During the early years of their careers, most of the women had encountered a paternalistic, often patronizing, majority male supervisor. He was likely to offer unsolicited advice in the "father knows best" tone, be overly concerned and protective about her physical well-being, or too eager to explain or defend her actions when she was perfectly capable of doing so herself. Most conferees had encountered one or more intelligent individuals in responsible positions who did not know that: (1) all women do not prefer to serve on the social committees; (2) all minority group members were not reared in ghettos; and (3) all women do not type and make coffee. As minority women they also frequently had to contend with insulting, insinuating social gestures and advances.

All of the women reported that they were expected to perform additional auxiliary functions, such as counseling students, serving on committees concerned with women's and/or minority interests, and administration of special programs for minority group members. While they were more than willing to give service in these areas, they deplored the fact that the additional physical and mental exertion and resultant professional disadvantage were not even acknowledged, let alone recompensed by the employer. Because of the traditional evaluation criteria for advancement, the scientists were forced to choose between their professional commitment to conduct research and publish, and their commitment to the interests of their minority group and/or women. To choose the latter was professional suicide. Private-sector employers were more likely than academics to look favorably upon non-professional involvement. Most, if not all, private and public sector employers reportedly look less favorably upon race or gender-related activity or political involvement than upon general-interest civic activity.

The Diversity of Race and Culture

The Native American conferees pointed out to others early in the conference that tribes differ, although all Native Americans share some values, concerns, and experience. It is a mistake, therefore, to think of all Indians as a monolithic group or to generalize from one tribe to another.

Strong dissatisfaction was expressed with the schools and programs operated by the Bureau of Indian Affairs (BIA). Unlike other minority schools, these were alienating rather than supportive institutions. Concern was also expressed that other government agencies excluded Native

American projects from consideration for funding because of the ill-founded notion that the BIA will provide for them. The Native Americans deplored the fact that they, more often than others, had to leave their communities to obtain quality education as well as health care and other services.

The Puerto Rican women, with one exception, had spent part of their formative years in Puerto Rico as a part of the majority ethnic group. This experience gave them somewhat different self-concepts than those of the other minorities present. When they came to the mainland, it was often traumatic to be treated as a minority group member. Even with their exceptional proficiency in English these women considered language difficulties a persisting problem. The quality of education for mainland Puerto Ricans was a source of great concern to the conferees. Because large numbers of Puerto Ricans tend to be found at the lower economic strata of mainland society, the quality of their education is below the standard of the average mainland or Puerto Rican school.

The Mexican-American scientists felt that cultural role expectations for women strongly influenced their self-concepts and behavior. It was difficult at times to disregard what they had been taught about appropriate behavior and roles. Women had been taught to give priority to family needs and concerns, to assume responsibility for most domestic functions, and to respect the authority of the man as head of the household and spokesperson for the family to the "outside world." In the lives of most of these women there had been a supportive man, either husband, brother, or father. Male Mexican-American scientists they had known were perceived, however, as much less supportive.

Black women have been strongly supported in their effort to advance by family, community, and minority institutions, including sororities. Traditionally, Black women have been urged by family and community to get as much education as possible. The assumption that, as adults, they would work was common to all. There was the sense that their brothers could survive regardless, but that girls needed education for protection and security. Compared to the sentiments expressed by some of the other ethnic groups, the Black scientists felt that their high levels of achievement in science had been a smaller source of alienation from their communities. The relatively greater numbers, urbanization and visibility of Black professionals, and the existence of Black teachers and institutions may have contributed to this difference.

Conclusions

These thirty women scientists from the Native American, Black, Mexican-American, and Puerto Rican communities have found their careers in science, engineering, and medicine to be exciting, demanding, and rewarding. Their extraordinary resilience and commitment have overcome the obstacles that have discouraged many others. While reluctant to concede their own specialness, the participants in this conference were unanimous in their determination to change things so that other able minority women need not be special to have careers in science. These minority women scientists desired also to share their experience with others

to help them understand and correct the forces that work against minority women. They hope that people will understand how being a minority woman has disadvantaged these scientists and will set about correcting the attitudes, systems, and behavior that have constituted the double bind.

Continuing Efforts . . .

Since the conference discussed above, the AAAS Office of Opportunities in Science has worked to implement the recommendations for change made by the minority women scientists. During a follow-up conference at the 1978 AAAS meeting, a communications network for minority scientists was established. The contact people for the network are:

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Selected Resources on Minorities and Women in Science

(Slightly modified from the conference report, "The Double Bind.")

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Mathematics is an entry-level subject that has prevented some women with scientific talent from utilizing their talents fully. Inadequate preparation in high school math may make remedial math necessary in college. In a structured scientific curriculum, this often means taking additional semesters. Desire to avoid this delay sometimes causes women to choose majors in nonscientific, traditionally female fields that require little or no math. As you read, consider your own reactions to mathematics and to sexist stereotyping.

MATHEMATICS AND SEX

by John Ernest

Excerpted from "Mathematics and Sex" (April 1976), Mathematics Department, University of California, Santa Barbara, published in American Mathematical Monthly 83 (8): 595-614.

The Inadequate Mathematical Training of Women in High School

In a perceptive article (40) entitled "Women in science: why so few?" Alice Rossi has described the many social and psychological influences which restrict women's choice of careers in science. (Cf. also (50).) In this section we shall describe another factor which restricts women's choices which, with some effort, may be more quickly corrected than the deep rooted causes of which Alice Rossi speaks. Whether through inadequate or even misleading counseling, or by personal choice, women simply are not taking enough of the optional mathematics courses offered in high school to prepare them to enter college programs in science, engineering or other "hard" disciplines. Without unusual and early efforts to make up this deficiency in their first year of college, all of these career options become effectively closed.

The sociologist Lucy Sells (cf. (41) and (42)) shared with us a fact sheet (dated December 15, 1973) which she developed on women in higher education at Berkeley and we quote here two of her points:

1. In a systematic random sample of freshmen admitted at Berkeley in Fall, 1972, 57% of the boys had taken four full years of mathematics, including the trigonometry-solid geometry sequence, compared with 8% of the girls. The four-year mathematics sequence is required for admission to Mathematics 1A, which in turn is required for majoring in every field at the University except the traditionally female, and hence lower paying, fields of humanities, social sciences, librarianship, social welfare, and education.

2. Among students earning the Bachelor's degree in the 21 largest letters and sciences departments, there is a strong and statistically significant relationship between having a one-year college mathematics requirement in the curriculum, and having less than one third of the degrees in the department earned by women.

This second result clearly underlines the tragic consequences of the incredible sex difference uncovered in the first finding. It is for this reason that Lucy Sells refers to mathematics as a "critical filter" in cutting down the percentage of women in many fields other than mathematics. Thus substantial changes in student and teacher attitudes towards girls taking mathematics, and particularly changes in the academic counseling procedures at the high school level, are likely to have a significant effect on the percentage of women majoring in disciplines which use mathematics as well as in mathematics itself. (Cf. (16)).

There is additional evidence that mathematics plays the role of a critical filter. For the period 1972-1975, the percentage of Ph.D.s earned by women is below 10% for the following fields: geography, astronomy, economics, mathematics, religion, computer science, applied mathematics, geology, agriculture, atmospheric science, business administration, physics, engineering and operations research (34). The only subject that does not fit the hypothesis, religion, is a very special case, since most ministers are men. In contrast women received more than 25% of the doctorates in the same period in each of the following fields: home economics, art history, romance languages, Germanic languages, comparative literature, social work, health sciences, English, speech, psychology, anthropology, library science, linguistics, education, classics, microbiology and sociology.

We found the result (1 above) of Lucy Sells' concerning the inadequate mathematical training of entering women students at Berkeley so remarkable that we decided to repeat the experiment at UCSB. We took a random sample of the files of freshmen (50 men and 50 women) who had been admitted to UCSB in the fall of 1973. Perhaps because this was done a year later (the situation is in great flux) and because UCSB is a rather different campus than Berkeley, we obtained somewhat different results. The percentage of entering men with four years of high school mathematics was considerably smaller (36%) while the percentage of entering women with four years of high school mathematics was somewhat larger (16%). Nevertheless the sex difference is still enormous (and statistically significant) and thus confirms Lucy Sells' findings at Berkeley. Other voluntary (and hence nonrandom) samples taken at UCSB also showed a significant sex difference in high school mathematical training. (We conjecture that these kinds of sex differences prevail at most schools throughout this country--and even the world.) (23)

We concur completely with the following Carnegie Commission recommendation:

Recommendation 7 (page 79 of (9)): Because of the evidence that many women enter college with inadequate mathematical training, special provision should be made to ensure that women desiring to major in fields calling for extensive use of mathematics are encouraged to make up this deficiency in order to enter the fields of their choice.

Specifically we recommend that every college and university offer, for credit, appropriate precalculus courses. This brings us to the next stage of our investigation, sex differences in mathematical education after high school.

Sex Differences in Mathematic Training at the University Level

We begin by looking for sex differences in the elementary calculus sequence beginning in the fall of 1971 and extending to the winter quarter of 1973, at UCSB. Math 34AB is the short (two-quarter) course while Math 34ABC, 4AB, is the long (five-quarter) course. (This long course branches in the second year. The figures for 4A include two other parallel sections, 5A and 4AH. The figures for 4B include 5B.)

We first note that the enrollment of women in these basic courses is disproportionately low. Women comprised only about a third of the class even though women were in the majority in the freshmen class of 1971. This undoubtedly is one of the inevitable corollaries of the deficient mathematical training women receive in high school, which we described in the previous section. Nevertheless we feel that improved counseling and other remedial aids at the college level can significantly improve these enrollment figures.

Enrollment, Fall 1971

	Freshman Class	Math 34A	Math 3A
Male	1001 (45%)	133 (63%)	328 (64%)
Female	1228 (55%)	78 (37%)	184 (36%)

We found no statistically significant sex differences in the grades achieved in the various courses. The Carnegie Commission ((91), pp. 50-51) has found that women tend to get better grades in college--in all fields. Nevertheless in every one of the five places (one in the short sequence, four in the long sequence) where a student has an opportunity to drop, the attrition rate was greater for women than men. The attrition rates for 34A → 34B, 3C → 4A and 4A → 4B were particularly large, with the women's attrition rate almost double that of men. This is particularly disconcerting as the original enrollment of women was already disproportionately low. These rather large attrition rates are difficult to interpret as various majors require only certain portions of the complete sequence.

The next table is read as follows: 33% of the men who completed 34A did not complete 34B. We did not actually follow each individual student through the sequence and hence the figures are subject to considerable error due to students transferring to other schools, and other students joining the sequence at a later stage.

The enrollment of women in the honors calculus section (which begins with the second quarter) is considerably lower than in the regular sections.

Attrition Rates for
Calculus Sequence, beginning fall 1971

Course	Male	Female
34A 34B	33%	58%
3A 3B	33%	37%
3B 3C	26%	35%
3C 4A	30%	51%
4A 4B	22%	41%
3A 3C	51%	59%
3A 4A	66%	80%
3A 4B	73%	88%

While women comprise about 35% of the second-quarter calculus enrollment, they comprise (as an average over the last four years) only about 20% of the honors section enrollment. The author taught this honors section in the winter of 1971 and there were nine men and no women enrolled. The following year the author was again scheduled to teach the honors calculus. This time he visited all the regular sections, described the nature of the honors section and specifically encouraged women to enroll. In the winter of 1972 the enrollment was again nine students, but this time there were six men and three women. We strongly recommend that every mathematics department look into their honors sequence and determine the percentage of female participation. We recently received a letter from Professor Joan Birman telling us of her similar experiences in the honors sequence at Barnard-Columbia (in alphabetical order).

I learned last year, to my astonishment, that for about four years running the honors calculus course had been all male, in spite of the fact that admission was based on an open competitive examination. This fall, one of the senior mathematics majors and myself made an intensive effort to encourage women to try the exam! The typical answer was, "I know I won't pass it,"--to which we replied over and over, "Well, if you try it, at worst you will confirm what you already know, and only an hour of time will have been

lost." After three days of such advising, the big day came, the exam was given, and this year the class has five men and five women!

These experiences indicate that we can anticipate considerable change in the enrollment of women in mathematics courses, if only a greater effort is made to encourage them to do so. A tiny survey was conducted, at Stanford, of women majoring in natural sciences, mathematics and engineering (cf. (12)). These women reported having received less encouragement to study mathematics than did any group of Stanford males, even those males who were majoring in history or the humanities. Numerous other studies confirm Joan Birman's experience, namely that a little encouragement can make a tremendous difference in female participation and performance levels in mathematics. (Cf. (6), (7), (8), (15), (19), (29), (33), (38), (47).)

We next examine the attrition of mathematics majors. The Carnegie Commission (9) has made the following observation:

When we consider the fields in which students major in college, we shall find that almost as large a percentage of women as of men receive bachelor's degrees in mathematical science, although women are considerably less well represented than men among recipients of degrees in fields calling for the use of mathematics as a tool, such as the natural sciences, engineering and economics.

The statistics on mathematics majors at UCSB do not seem to follow this national pattern. We have followed the class of '72 and the class of '73 and found that women got less than half as many degrees in mathematics as men. Here again we find rather heavy attrition. For example, when the class of '72 began in the fall of '68, about the same percentage of men and women indicated their desire to major in mathematics. (70 women and 63 men indicated their intention to major in mathematics. This represents 4.4% of the women in the class and 4.5% of the men.) These percentages continually decline in the following years, the attrition being higher for women than for men. Finally at graduation time there were only 20 women mathematics majors (1.8% of the women graduates) and 48 men mathematics majors (3.1% of the men graduates). For the class of '73 the pattern is the same. Only 16 women (1.5% of the women graduates) and 45 men (2.8% of the men graduates) received bachelor's degrees in mathematics. Of course the attrition figures may be partially caused by students transferring to other campuses of the UC system.

We conclude that it is very much in the university's own interest to invest in subsidized child-care centers, better academic and career counseling, and more flexible degree programs, among other things being recommended by various women's groups and the Carnegie Foundation (9) to try to decrease this sizable attrition among women.

In going from the bachelor's degree into a graduate program we once again find that attrition takes a higher toll for women than for men. (At this point we may marvel that there are as many women mathematicians as

there are!) For example, Lucy Sells (41) has found that "there is a statistically significant drop in the proportion of women earning the bachelor's degree in the physical sciences at Berkeley (26%) and the proportion of women applying to graduate school in the physical sciences (11%)." In the fall of 1972, 15% of the applicants to the graduate program in mathematics at UCSB were women, while in 1973 it was 16%. Averaged over these two admission periods, the acceptance rate was 78% for the men and 88% for the women.

We feel an effort can and should be made to increase the proportion of women in both the undergraduate and graduate mathematics program. For example, women students at MIT compiled an excellent booklet about the school's opportunities for women and mailed it to 10,000 female high school juniors. This resulted in 1,400 inquiries from women about admission to MIT, nearly four times as many as the year before. The engineering department at Berkeley has also used a special recruiting program, with some success. At Mills College, an all women's school in Oakland, California, Lenore Blum found that the mathematics enrollment there nearly doubled after she made a concerted effort to encourage women to enter science, including the development of special programs, counseling and publicity (6).

Considering the importance of encouragement, the dearth of women faculty in mathematics, physics, chemistry and engineering can only have an inhibiting affect on female enrollments in these subjects. The mathematician Martha K. Smith has written us concerning this matter.

Role models play an often neglected but, to my mind, important role in education. If a young girl never encounters a woman in mathematics, it is quite reasonable that she should conclude that mathematics isn't something women do (whether for lack of ability or lack of opportunity). Perhaps more likely, she may not even think of the possibility of a woman mathematician.

I know I felt a certain amount of relief when, the summer after my first year in graduate school, I met a woman mathematician for the first time. There was living evidence that what I wanted to do was not impossible.

To reverse these stereotypes won't be easy, but I think it's possible. An effort should be made to get all the trained women mathematicians possible into positions where they will have an impact on both male and female students and colleagues. Women need to see examples of practicing mathematicians: men need to become accustomed to accepting a woman as an honest-to-goodness colleague rather than a curiosity.

Women in the Mathematics Profession

We have been most fortunate that a large number of women mathematicians have corresponded and shared with us their experiences, attitudes and insights. They have enabled us to go beyond statistical averages to see the complexity and variability of human personality and creativity. Women, just like men, have various reasons for being attracted to mathematics, few of them having anything to do with their sex. They also hold diverse opinions regarding the various matters considered in this report. Their suggestions have done much to shape our study. Their ideas permeate this report. We therefore wish to express our deep appreciation to the following women mathematicians who have consulted with us: Joan Birman, Judy Bruckner, Mary Gray, Susan Gerstein, Mary Elizabeth Hamstrom, Eleanor G. Jones, Nancy Kopell, Tilla Milnor, Cathleen Morawetz, Alice Schafer, Marianne Smith, Martha K. Smith, Ann Stehney, Diane Stuebing, Olga Taussky Todd, Karen Uhlenbeck.

Just over half of our respondents indicated they had experienced some form of discrimination in their professional lives. Essentially, all of them indicated they had experienced sexist attitudes of some form.

Professor Martha Smith (whom we have already quoted) has put forth the idea that many women are dissuaded from a mathematical career by societal stereotypes:

Many people on hearing the words "female mathematician" conjure up an image of a six-foot, gray-haired, tweed suited, oxford clad woman. . . . This image, of course, doesn't attract the young women who is continually being bombarded with messages, direct and indirect, to be beautiful, "feminine" and to catch a man.

We have found the notion that women who excel in mathematics are less feminine to be utter nonsense. For example, in Ravenna Helson's study (20) of creative women mathematicians, her findings did not "show the creative woman to be more masculine, if one means by this that they might have been expected to score higher on measures of masculinity-femininity, or dominance, assertiveness, or analytical ability." In another (older) study (30) involving undergraduate students at UCLA, Philip Lambert found that among women students "mathematics majors were not only equal to non-mathematics majors in femininity, but significantly more feminine." This finding was sufficiently surprising that Lambert took a second sample, which confirmed the result.

Joan Birman, a professor at Columbia University, was influenced earlier in her life by the "unfeminine" stereotype. "Why didn't I study mathematics at age 21? I felt it was not a 'feminine' thing to do. I'm afraid that it seems to me that this is a continuing problem for many young women." As we have just noted, studies indicate that, if anything, women mathematicians tend to be more feminine than the average. We thus feel it is time for society to completely do away with the ridiculous prejudice that a female mathematician is somehow less of a woman because of her intellectual pursuits.

When discussing the stereotype of a female mathematician, colleagues sometimes refer to Emmy Noether. (Cf. (11), (25), (48), and (49).) Since she died the same year I was born I have no firsthand knowledge of her traits. In making her way to the top of a male-dominated profession, she may not have met everyone's expectations of the way a female should look and behave. Herman Weyl described Emmy Noether quite candidly in the eulogy he wrote for her: (37)

It was only too easy for those who met her for the first time, or had no feeling for her creative power, to consider her queer, and to make fun at her expense. She was heavy of build and loud of voice, and it was often not easy for one to get the floor in competition with her. She preached mightily, and not as the scribes. She was a rough and simple soul, but her heart was in the right place. Her frankness was never offensive in the least degree. In every day life she was most unassuming and utterly unselfish; she had a kind and friendly nature. Nevertheless she enjoyed the recognition paid her; she could answer with a bashful smile like a young girl to whom one had whispered a compliment. No one could contend that the Graces had stood by her cradle; but if we in Gottingen often chaffingly referred to her as "der Noether" (with the masculine article), it was also done with a respectful recognition of her power as a creative thinker who seemed to have broken through the barrier of sex."

It was the Russian mathematician P.S. Alexandroff who had dubbed her "der Noether." Nevertheless Alexandroff, who knew her well, spoke eloquently of her feminine characteristics, in an address to the Moscow Mathematical Society in 1935:

A spokesman of the most abstract areas of mathematical science, she distinguished herself at the same time by a surprising sensitivity in understanding the great historical movements of our epoch; always vitally interested in politics, hating war with her whole being, and hating chauvinism in all its manifestations, she never in the area knew any vacillation.

Such was Emmy Noether, the greatest of women mathematicians, a great scientist, an amazing teacher, and an unforgettable person. True, Weyl had said that "the Graces did not stand at her cradle," and he is right, if one has in mind the generally known heaviness of her appearance. But here Weyl is speaking of her not only as a great scholar, but also as a great woman. And she was that --her femininity appeared in that gentle and subtle lyricism which lay at the heart of the far-flung but never superficial concerns which she maintained for people, for her profession, and for the interests of all mankind. She loved people, science, life, with all the warmth, all the cheerfulness, all the unselfishness, and all the tenderness of which a deeply sensitive--and feminine--soul is capable. (25)

This problem of the "unfeminine" stereotype of the female mathematician is only one of many faced by the woman considering mathematics as a career. Professor Joan Birman has discussed other misunderstandings with us:

The female students I know also seem to often misunderstand the nature of mathematics. It appeals to them (I think) because they are systematic, neat, logical and orderly. They do not seem to appreciate the creative, imaginative, aesthetic aspect of mathematics, and indeed are often bewildered by it when they first encounter advanced courses. I hope this is something which is changing, as more young women become aware of themselves as questioning, growing, thinking people. Conformity is really deadly for mathematics, because it is not possible to learn the subject unless you are constantly questioning whether you understand it. To become a mathematician it is also necessary a) to have a good deal of tolerance for frustration, b) to not be distressed by the distrust or fear other people have of those who are "smart," and c) to not be afraid to be wrong or make mistakes--all of these seem to be problems for women.

There are additional accounts of the experiences of women in the mathematical profession: Sonja Kovalevsky (26), a nineteenth-century Russian mathematician; Else Hoyrup (22), a Danish mathematician; and Marian Boykan Pour-El (29), a professor at the University of Minnesota, have all written about their professional development. Clearly, given the circumstances described in this report, women have had to have considerable courage and motivation to become creative mathematicians. This was recognized by Karl Friedrich Gauss, who wrote the following words to the great French mathematician, Sophie Germain (1776-1831).

But when a person of the sex which, according to our customs and prejudices, must encounter infinitely more difficulties than men to familiarize herself with these thorny researches, succeeds nevertheless in surmounting these obstacles and penetrating the most obscure parts of them, then without a doubt she must have the noblest courage, quite extraordinary talents and a superior genius. (37)

In the past, women have had to suffer considerable employment discrimination. Even when Emmy Noether finally obtained the position of "nichtbeamteter ausserordentlicher Professor" at Göttingen in 1922, she still was not paid a salary. She received only a modest remuneration for some teaching in algebra (37). Fortunately, we are beginning to see some very healthy changes in the employment situation for women mathematicians. The nepotism rules, invariably discriminatory against the wife, are rapidly falling away. Under the watchful eye of the Department of Health, Education, and Welfare, academic and industrial institutions are adopting widespread procedures to ensure that hiring patterns are free of sex bias. We strongly support such affirmative action to ensure that all

potential female candidates are considered. Such increased recruitment efforts can only enlarge the list of qualified candidates and thus result in the raising of standards. . . .

We would like to conclude with a short discussion of certain moral issues which have been lying beneath the surface of this study. The question is this: Have we made an implicit assumption that dropping out of a degree program in general, and a mathematics program in particular, is necessarily bad? Given the extremely depressed (and depressing) employment prospects for young mathematicians (both men and women) perhaps those who drop out, or don't even begin the training process, are in fact making the correct choice, not only in terms of economic consequences, but also in terms of fulfilling their human potential.

We do not claim that the goal is the elimination of all measurable sex differences in all human pursuits. We all have different opinions on the extent to which that would be desirable. But we are all agreed that the many sex differences in mathematical training and attitudes described in this report are not the result of free and informed choice. If they were then the low enrollments and high attrition rates for women in mathematics would be a matter of less concern. The immorality of these sex differences lies precisely in the fact that they are the result of many subtle (and not so subtle) forces, restriction, stereotypes, sex roles, parental-teacher-peer group attitudes, and other cultural and psychological constraints which we haven't begun to fully understand. Before we can hope that each individual child and young adult will make these choices freely and wisely we must work towards a society generally (and an academic program specifically) which ensures that the freedom of opportunity to become whatever that individual is truly capable of becoming is not compromised by such a chance event as the child's sex at birth. We believe that each of us, male or female, who have children or anticipate having children, would desire such a society for them. And those of us who are mathematicians and whose lives have been so greatly enriched as a result would wish that this opportunity be available, regardless of sex, to anyone who finds they have the aptitude, interest and creative and intellectual ability to do it.

Albert Einstein, on the occasion of the death of the mathematician Emmy Noether in 1935, wrote the following lines in a letter to the New York Times. (See (25) for the full text of this beautiful letter.)

Beneath the effort directed toward the accumulation of worldly goods lies all too frequently the illusion that this is the most substantial and desirable end to be achieved; but there is, fortunately, a minority composed of those who recognize early in their lives that the most beautiful and satisfying experiences open to human kind are not derived from the outside but are bound up with the individual's own feeling, thinking and acting. . . . However inconspicuously the lives of these individuals run their course, nonetheless, the fruits of their endeavors are the most valuable contributions which one generation can make to its successors.

We believe that we must continue the effort to bring about a sexually unbiased social and intellectual environment where many other Emmy Noethers will be able to flourish and grow. We will all be the beneficiaries of their creative endeavors.

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Florence
Sabin



Nature of Science

Introduction

What is science? From the viewpoint of philosophers it is a way of obtaining knowledge, and philosophers wrestle with the difficulty of defining knowledge, as well as specifying the logic and methods by which scientists work. In this context, science is an intellectual activity performed by educated individuals. Science can also be considered to be information--information that has been produced by some scientists, tested by others, and then accepted into the general consensus. From the viewpoint of sociologists, historians, and social commentators, science is a human activity closely related to technology and strongly interacting with other human activities and the evolution of civilization. In this context, science can be viewed as a social system in which new information is produced in return for recognition. For many Americans, science is dimly perceived as the expensive activity of a strange group of people who produce implements that are useful or dangerous, or both. For those who practice science, it is a source of intellectual delight, frustration, hard work, and a job, in varying combinations at various times.

The following articles elaborate on some of those views of science. Although the nature of science is a complex topic that cannot be treated adequately within the confines of this book, further insights can be gained by reading biographies of scientists and accounts of scientific investigations. Keep in mind questions such as: What do scientists do? What methods do they employ? What are the roles of logic and intuition? What are the consequences for society of the work of scientists?

The Process of Discovery

In this subsection we examine science as a way of obtaining knowledge, and we illustrate the process of discovery and also personal interactions involved by considering a specific example, elucidation of the structure of DNA.

ESSENTIALS OF THE SCIENTIFIC METHOD

Scientific study of a phenomenon begins with the making of observations, either deliberately or incidentally in connection with some other question. When enough information is available, a hypothesis is formulated. On the basis of that hypothesis, experiments are designed to test the hypothesis. Some would say that scientists cannot prove anything, but they can show only that hypotheses are false. Alternatively, scientists make observations or perform experiments that may give results consistent with the hypothesis but are not capable of disproving it. A hypothesis may be elaborated and tested in two ways: by making numerous observations, or by taking an intellectual leap from a few data and constructing an actual or mental model of the phenomenon under investigation. Both data collection and model-building are necessary activities in science, but individuals differ in their inclination to do one or the other and in their opinions about the amount of information required before model-building is appropriate. After the data have been assembled and analyzed, the final step in a particular study is publication of the results.

Communication among scientists is an important component of the process of discovery. Sharing ideas and results, in print or in conversation, allows one person to build on the work of another and thus to see further. As a consequence of the interactions among scientists and their work, science as a body of knowledge is increased, modified, and, most importantly, corrected of errors. Error correction is particularly important because observations and the conclusions drawn from them may be incomplete or even wrong. There is an element of personal interpretation in the most objective process. Most scientists are influenced by the prevailing views of what is correct or important in their field of study, and they work accordingly. The prevailing view, or "paradigm," provides ideas for further work. At some point the "paradigm" is no longer sufficient to deal with new data, or it is shown to be clearly incorrect. Then a new paradigm is generated, and the old is overthrown in a "scientific revolution" (Kuhn, 1970).

Scientific discoveries range in significance from major breakthroughs leading to a new paradigm to trivial details mouldering unused in dusty library volumes. A scientific discovery is significant if it is surprising; that is, if the discovery provides an answer that was not predicted on the basis of previous understanding (although in retrospect it may be obvious). It is significant if it provides a new perspective that leads to additional experiments and discoveries. It is significant if it and subsequent related discoveries have a marked impact on some aspect of society. Some discoveries are promptly hailed as significant; others come to be appreciated

only after additional progress has given them a context and a theoretical or practical usefulness. Sooner or later, however, the significance of a particular discovery wanes, as new discoveries envelop and extend beyond the old result.

This discussion of the process of discovery has touched upon two aspects of doing science: the procedures of a single investigation and the incorporation of individual results into a generally accepted body of facts and relationships. Almost all of the statements made above can be challenged and amplified; further discussion will be found in the references listed at the end of this section.

In addition to intellectual activity, with its logic and abstractions, the process of discovery has an emotional side. It is an activity of individuals who want, in varying degrees, to be right, famous, respected, honored, well paid, a recognized authority, and supervisor of an impressive laboratory in which other people do most of the work. These emotional impulses sometimes give rise to competition for priority in publication; competition for more salary, grants, and space; adherence to a pet idea or experimental methods; failure to acknowledge the contributions of other scientists; and intellectual aggression or obsequiousness, as the occasion demands. Science offers rewards other than the intellectual pleasures of scientific discovery (discussed in the Status and Rewards subsection), and it is not surprising that these additional rewards are sought.

In accordance with the requirements for success of the scientific method, and as a check on emotional and competitive impulses, the behavior of scientists is influenced by an unwritten ethical code. Scientists are expected to report their experiments and results completely and honestly. They are expected to acknowledge the help of others. They are expected to evaluate results and theories on their merits, independently of personal friendships or animosities among the scientists involved. How well these expectations are fulfilled is a matter of debate. Gross dishonesty is a rare horror and generally offensive. Matters of judgment are more difficult to evaluate. For example, is the acknowledgment sufficient? Are A's results ignored by B because B can't stand A? Sometimes the ethical boundary is inconclusively fuzzy.

These generalities about science are illustrated to varying extents in the example that follows and in other biographies of scientists.

ELUCIDATION OF THE STRUCTURE OF DNA

by Ann E. Kammer

Introduction

Various aspects of the process of doing science are illustrated by the elucidation of the molecular structure of the hereditary material deoxyribose nucleic acid, or DNA. For many years nucleic acids were thought to be simple and relatively unimportant molecules in the cell nucleus, whereas proteins, which are more diverse and complicated molecules, were thought to carry the hereditary information. Thus attention was given to proteins, and little research was done on nucleic acids. Gradually an appreciation of the importance of nucleic acids developed, as a result of the work of many individuals, gathering new information and building models. As part of this development, the discovery of the molecular structure of DNA was enormously significant. With respect to the criteria summarized in the previous subsection, it was a major breakthrough that, along with other developments in molecular genetics and biochemistry, led to a new and rapidly growing understanding of cell function and replication. The proposed structure was compelling, beautiful, and in some respects surprising. It prompted numerous experiments of both theoretical and practical importance. It provided a testable hypothesis about gene duplication and led to an understanding of the genetic code. It unified previously available but disparate data from biochemistry, genetics, microbiology, and cytological studies of chromosome behavior into a coherent picture of the mechanisms of heredity. These and related results made possible studies of the mechanisms of mutation by radiation and by chemical mutagens and raised the possibility of correcting genetic defects. They also led to an understanding of the mechanisms by which viruses infect cells. These and related discoveries will probably lead to a resolution of that as yet mysterious disease, cancer. The knowledge acquired and the numerous discoveries that followed the initial elucidation of the structure of DNA also provide potential power to manipulate the genetic material, and consequently there are now growing social concerns about the possible uses of this power to remedy genetic defects and to create new, virulent plagues.

The following story, which describes the discovery of the molecular structure of DNA, was chosen to illustrate (a) the process of scientific discovery, (b) the adherence to preferred methods, (c) the importance of communication among scientists, (d) the personal interactions that influence the process of discovery, and (e) the difficulty of sorting out the truth about causal relationships in historical events. Since this material is a specific example that illustrates some generalizations about science and scientists, it should be read with attention to both generalizations and details. Some questions for contemplation are included after the text.

The story focuses primarily on four scientists: James Watson, Francis Crick, Rosalind Franklin, and Maurice Wilkins. Watson and Crick were working at Cambridge University while Franklin and Wilkins were at King's College, University of London. The problem they confronted in 1951 was,

what is the structure of DNA? DNA contains four different organic bases, sugar, and phosphate groups; how are these subunits arranged? How is the structure duplicated so that it can be passed to successive generations? Rosalind Franklin and Maurice Wilkins were both, albeit independently, studying the structure of DNA by means of X-ray crystallography. In this technique a beam of X-rays is diffracted by a small crystal, and the resulting image is recorded on photographic film. Diffraction images are obtained in several directions from the crystal, and then by careful analysis and calculation, the three-dimensional arrangement of molecules within the crystal can be determined. Crick was also proficient in this technique, but he was using it primarily to study the structure of proteins. While James Watson was at Cambridge, he learned the essentials of the technique from Crick and with the assistance of other scientists there used it to study a virus. As the following account will show, Rosalind Franklin's X-ray crystallography provided essential data about the structure of DNA. Watson and Crick used these data and other information in their efforts to construct a three-dimensional, physical model of the molecule.

The following accounts are based on The Double Helix, (1) James Watson's personal story of the discovery written in 1968, and on a biography of Rosalind Franklin written in 1975 by her friend Anne Sayre. (2) For logistic reasons, we have attempted to convey Watson's account accurately and briefly in our words. We recommend, however, that you read his book and compare it with Sayre's account and with other versions of the events leading to the discovery of DNA as you attempt to evaluate this example of the scientific process and the history of science. Note that in the following account we have interdigitated two versions, Watson's and Sayre's.*

The Principal Characters and Their Roles

The story begins with the meeting of Watson and Crick in Cambridge, England:

In the fall of 1951, James Watson, a former whiz kid from Chicago, who at twenty-three already had earned a Ph.D. in biology from the University of Indiana, arrived at the Cavendish Laboratory in Cambridge, intent on cracking the structure of DNA. There he met Francis Crick, a bright and self-confident Englishman, who at thirty-five was still trying to finish up his Ph.D. Crick has said that Watson "was the first person I had met who thought the same way about biology as I did. . . . I decided that genetics was the really essential part, what the genes were and what they did. And Watson was the first person I had met with exactly the same ideas I had. . . ." (3) To Watson and

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Crick, DNA held the secret of the gene, the unit of heredity in living organisms. It was not just a molecule; it was the golden molecule that promised a Nobel prize. (4)

Watson and Crick could not begin to work on DNA immediately. Crick was busy with his studies of proteins, and others at the Cavendish Laboratory were not particularly interested in nucleic acid. Furthermore, it was understood that, in England, studies of the molecular structure of DNA were the "personal property" (5) of Maurice Wilkins at the University of London. Both Wilkins and Crick used the technique of X-ray diffraction to probe the structure of molecules, and they frequently exchanged ideas and information. It would have been awkward, if not unethical, for Crick to begin work on Wilkin's research problem.

In Watson's eyes there was another difficulty impeding progress toward understanding the structure of DNA. Wilkins was distracted and upset by his new assistant, Rosalind Franklin. According to Watson, Maurice Wilkins, "a beginner in X-ray diffraction work, wanted some professional help and hoped that Rosy, a trained crystallographer, could speed up his research. Rosy, however, did not see the situation this way. She claimed that she had been given DNA for her own problem and would not think of herself as Maurice's assistant." (6) Nor was "Rosy" likely to "calm down" and conform to the helpmate role. As Watson saw her, she chose not to emphasize her feminine qualities, but had straight hair, no lipstick, and dowdy dress. She chose to live the dedicated, austere life of an unmarried woman scientist, and she had her own professional goals, which did not include being Wilkin's research assistant. "Clearly Rosy had to go or be put in her place." Not only was she uncooperative, her belligerent moods interfered with Wilkins's maintaining a "dominant position that would allow him to think unhindered about DNA." (7) However, there was no reasonable way Wilkins could get rid of "Rosy," and it was possible that her knowledge and skills could help him "if she could only keep her emotions under control."

It is important to examine in more detail who this "Rosy" was that Watson described in The Double Helix. According to Hubbard:

Rosalind Franklin was an exceptionally talented and productive young physical chemist. Educated at Newnham College, Cambridge, she worked during World War II with the British Coal Utilization Research Association and in 1947 moved to Paris, where during four happy years she was trained in X-ray diffraction while working on the structure of carbons. Under pressure from her family to return to England, she applied for a Turner-Newall Fellowship in order to join Professor Randall's unit at King's College, London, where she arrived in January 1951. From conversations and correspondence with Randall she had understood that she was to start by developing an X-ray camera with higher resolving power than was then available at King's, in order to study the structure of DNA. The previous June, Maurice Wilkins at King's had received from a colleague a

sample of DNA, with which a graduate student named Gosling had obtained a very encouraging X-ray diffraction pattern. Wilkins was away when Franklin arrived and, upon his return, found her fully engaged in her new work, but in no way eager to share it with him.

Anne Sayre has this to say:

[Watson] presented in The Double Helix a character named "Rosy" who represented, but did not really coincide with, a woman named Rosalind Franklin. . . .

"Rosy" was not recognizable as Rosalind Franklin. She was recognizable as something else not related to the facts.

"Rosy" was less an individual than a character, in the exact sense, that is, of a character in a work of fiction. . . . The Double Helix was not a work of fiction, but an account--albeit personal--of certain events in history in which a number of real people had been concerned. And of these people, one who was no longer living had been curiously transformed into what she had not been in life.

Both the method of the transformation and its results were clear enough. The technique used to change Rosalind Franklin into "Rosy" was subtle, but really not unfamiliar; part of it, at the simplest level, was the device of the nickname itself, one that was never used by any friend of Rosalind's, and certainly by no one to her face. But beyond this, a series of minor inaccuracies of fact, each aligned in a consistent direction, and accompanied by an unvarying emphasis in minor details, served to form a character out of a real individual by a means not materially different from the one a novelist uses in creating a character out of the whole cloth. More important than how it was done was the result achieved. If Rosalind was concealed, the figure which emerged was plain enough. She was one we have all met before, not often in the flesh, but constantly in a certain kind of social mythology. She was the perfect, unadulterated stereotype of the unattractive, dowdy, rigid, aggressive, overbearing, steely, "unfeminine" bluestocking, the female grotesque we have all been taught either to fear or to despise.

Whether this grotesque exists anywhere in real life, or is truly a figment of myth and imagination, is beside the point. Certainly she did not exist in Rosalind. By masterly technique, which deserves demonstrating, a magical change was made that transformed the real into the fictive. Watson introduced his "Rosy" early in his book, in a long descriptive passage remarkable for the number of small errors of fact which it embraced, and for the negativism of all its characterizing phrases. It is here that we are told, at the outset,

a true fact, which is that Rosalind was working in 1951 in a laboratory at King's College (London), and then, at once, an untrue one, which is that this laboratory was under the direction of Maurice Wilkins. We are confronted, that is, with a trivial error that appears barely worth correcting.

But wait. The stage has been set, and with great economy, for we have been informed, in this very easy way, that Rosalind was working for Wilkins; we are even assured that her presence in that laboratory was intended only to speed up Wilkins's research. The relative positions of two people have been defined, and with what follows, the "Rosy" character instantly emerges. . . .

But the picture offered rests upon air; the facts contradict it. The organization at King's College in which both Rosalind and Maurice Wilkins worked was under the direction of Professor (now Sir John) Randall, and in his laboratory they were on a level of equality, each supervising a group--in Rosalind's case, a very small one--engaged in entirely different research activities, though both were concerned with DNA. The DNA problem, if it belonged to anyone, belonged to Randall's laboratory. One aspect of the research being done upon it was certainly Rosalind's responsibility, for this approach made use of X-ray diffraction methods, and this was a technique which she commanded that Wilkins at the time did not. She had, in fact, been brought into Randall's organization in the first place in order to organize, supervise, and carry out that aspect of the DNA work, while Wilkins specialized in biochemical and biophysical studies. And suddenly it appears that the young woman who refused to think of herself as Maurice Wilkins's assistant was quite justified in her reluctance, for this she never was, nor was ever meant to be.

[Franklin's position has been recorded in her own words, cited by Sayre.]

"During 1950, M. H. F. Wilkins succeeded in obtaining well-oriented fine fibres from a specimen of desoxyribose nucleic acid prepared by Professor Signer [Berne, Switzerland], and R. G. Gosling showed that a bundle of these fibres gave X-ray diagrams of exceptional quality. In January 1951 it was agreed that I should undertake, in collaboration with Gosling, a systematic X-ray investigation of these fibres." So Rosalind wrote in February 1952, in a report to Randall covering the progress of her first year's work at King's College. It is a modest statement in which Wilkins and Gosling are given their due. It is modest also in that Rosalind named herself, in connection with Gosling, who was then a graduate student, simply as a collaborator. Surely this makes a little puzzling the suggestion that

Rosalind, in working on DNA, misunderstood the role planned for her at King's and took over what she was not intended to do.

But what she had to do first was to set up a laboratory. King's was not well equipped for X-ray diffraction work of the sort needed for DNA. . . . "Gosling's original X-ray photographs were taken with standard apparatus . . . not well suited to this type of work. The diffraction pattern of DNA, like that of proteins, is confined to rather small angles . . . and the amount of information that can be obtained from a fibre diagram is, therefore, to a large extent determined by the X-ray-optical resolving power of the system used. An X-ray tube having a small focus with high intrinsic brilliance is required." . . . In fact, as Rosalind's report goes on to say, "the greater part of the first eight months was taken up with the assembling of the necessary apparatus."

Sayre continues:

[According to Watson] "Rosy" is something of a termagant; mere inspection by Watson indicates that she is willfull and unbending. By choice, he tells us, she refuses to emphasize her feminine qualities--by which he means (and it is his idea of femininity) that she is badly dressed, wears no lipstick, does nothing interesting with her straight black hair. As "Rosy" is the model of the bluestocking, of course she peers out at the world from behind her spectacles, and we know she does because Watson muses upon how she might look if ever she removed them. (And those spectacles are, as it happens, rather crucial.) He is kind enough, however, to want to find excuses for her being as she is and, therefore, conjures up an unsatisfied mother hovering somewhere in the wings who has urged a professional career upon her daughter, hoping to save her from a dull marriage; he is surprised to find that this really cannot be the explanation for "Rosy's" defects, because no pushing mother can be located in a comfortable, prosperous background, and therefore regretfully concludes that "Rosy's" austere qualities are all of her own making.

Now, all of this is thoroughly absurd. It is absurd in general. It can never be of much importance how any scientist, male or female, chooses to dress, and few of them of either sex are noted leaders of fashion; it can never matter how any scientist looks, and few of them are ever likely to be confused with film stars, nor is there any reason why they should be. But it is just as absurd in particular. To like or dislike anyone's style of dressing is a matter of taste. People with whom Rosalind Franklin worked in both England and in France thought her rather

smart, always well-groomed, discernibly English in her style, but far from habitually dowdy. The unadorned lips that Watson found distressing simply indicate him to be unobservant--the lipstick was almost invariably there. And of all things, he cannot really have wondered often or long how Rosalind would look without spectacles, for he cannot often have seen her wearing any. Rosalind had the eyesight of an eagle and resorted to magnifying lenses only for the closest of fine work.

Communication Among Scientists

Communication with other scientists and the resultant access to information and advice played important roles in Watson's intellectual growth and in the development of ideas about DNA. One of the differences between Watson's situation in Cambridge and the environment in which Rosalind Franklin worked in London was the opportunity each had to exchange ideas and information with other scientists. Franklin worked with a graduate student, R. G. Gosling, but personality clashes blocked almost all constructive interactions between Franklin and Maurice Wilkins. In contrast, Francis Crick and James Watson exchanged information freely. There was no obstacle preventing them talking several hours each day. They had the time, they shared an office, and their personalities were compatible. Watson taught Crick about viruses and genetics, and Crick taught Watson about crystallography and, in particular, about Pauling's discovery of the helical conformation of certain proteins. Linus Pauling was a famous chemist at the California Institute of Technology and an authority on chemical bonds. Watson perceived that Pauling had succeeded in discovering the three-dimensional structure of proteins by building a physical model of his protein, rather than by concentrating on accumulating and analyzing data from X-ray crystallography. Building a model of this kind entails looking for relationships. It involves playing with toys: paper, plastic, or metal cutouts for molecular groups of different shapes and rods for the bonds that connect them. Watson decided that model-building was the approach he would use on DNA. He was well aware that Pauling was a potential competitor in the race for the golden molecule and the Nobel prize with which it was associated. Pauling was interested in DNA molecules and had written to Maurice Wilkins, asking about his X-ray data, which Wilkins declined to share. Watson's impatience to begin work on DNA was fueled by the thought of competition from Pauling, who was unhindered by the conventional niceties that a colleague in another laboratory was working on the problem and who undoubtedly did not suffer as Maurice Wilkins did with a contentious "Rosy."

Multiple Sources of Information

Making a scientific discovery often requires that an individual or a team obtains and integrates information from diverse sources. Building a molecular model requires a certain knowledge of chemistry and some specific information about the molecule to be constructed. Watson and Crick brought to their joint efforts to devise a molecular model of DNA

their previous experience in biochemistry and genetics and in X-ray crystallography. During the time they were working on the model, Watson and Crick used Pauling's influential book, The Chemical Bond. Other sources were a book, The Biochemistry of Nucleic Acids, by J. N. Davidson, and published scientific papers on the biochemistry and X-ray crystallography of DNA. An important chemical fact about the organic bases was contributed by an American scientist, Jerry Donohue, who by chance was occupying a desk in their office at the Cavendish Laboratory and who, we shall see later, was important in their success.

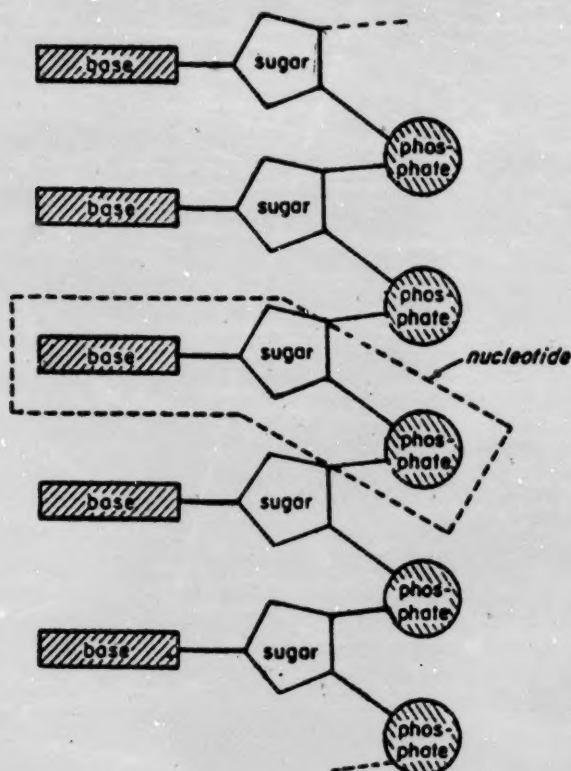
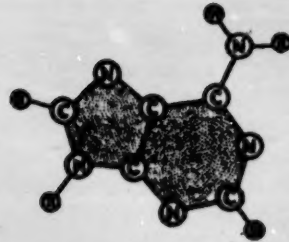


Figure 1. Nucleotides, composed of an organic base, a pentose sugar, and a phosphate group, linked via the phosphate groups in a long chain (redrawn from The Double Helix).

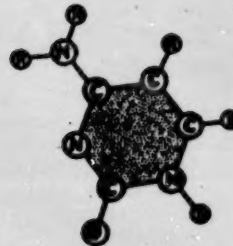
In addition to these more general reference sources, essential specific data came from the laboratories of Maurice Wilkins and Rosalind Franklin. It is a matter of some interest to determine how much data Watson and Crick obtained from these two people, how important these data were for their construction of the model, and by what means the data were obtained. Some information on these points is provided by Watson in The Double Helix, and Anne Sayre attempted to evaluate the evidence in her book, as we shall see as the story continues.

PURINES

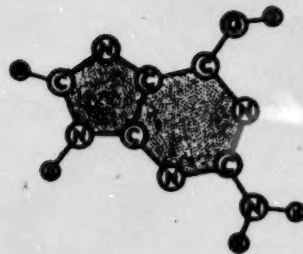
PYRIMIDINES



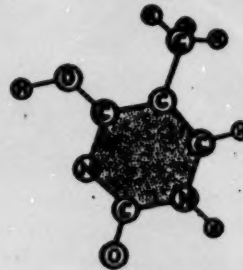
adenine



cytosine



guanine



thymine

Figure 2. The four organic bases in DNA (redrawn from The Double Helix).

DNA: Watson and Crick's First Model; Franklin's Data

In 1951, when Watson and Crick began to construct their DNA model, they made certain assumptions to facilitate their task. They assumed that the DNA molecule had an uncomplicated helical structure. They also assumed that the large number of nucleotides of which the molecule was composed were attached together in a long, regular chain. A nucleotide consists of an organic base connected to a sugar that bears a phosphate group (Fig. 1). The organic chemists in another Cambridge laboratory thought that the nucleotides were linked to each other via the phosphate groups, but this structure had not been confirmed experimentally. The nucleotides in DNA were known to be of four types, differing in the organic base. Two are purines called adenine and guanine, and two pyrimidines, cytosine and thymine (Fig. 2). In the long-chain molecule of DNA, the sugar-phosphate portion forms the "backbone" from which the bases, planar molecules because of their ring structure, are suspended (Fig. 1). Watson and Crick assumed that the backbone was a regular

structure, because that was the simplest hypothesis and because some regularity was required to explain the crystal structure that gave ordered X-ray diffraction patterns. The X-ray data conveyed to Crick by Wilkins suggested that the DNA molecule was thicker than expected if it contained only one chain of nucleotides. Two questions then arose: How many chains are present, coiled together to make the molecule? What kinds of bonds hold the chains together--hydrogen bonds between the bases or ionic bonds involving the negatively charged phosphate groups? The former would require that the bases lie in the center of the molecule, with the sugar-phosphate backbone on the outside. The latter would require that the phosphate groups (and some positive ions, such as sodium or magnesium) lie in the center, with the organic bases exposed on the surface.

According to The Double Helix, before Watson and Crick could make much progress on their model-building, they needed some essential data about the three-dimensional structure of the molecule. Such data came from X-ray diffraction studies. They know of one published paper on their topic, by W. T. Astbury, and the picture was five years old. Maurice Wilkins presumably had better pictures, which could speed Watson and Crick's efforts at model-building. They invited Wilkins to Cambridge for a weekend and were reminded that he was already thinking about helices. At this time Wilkins thought that the DNA helix contained three polynucleotide chains. Wilkins also thought that more data were needed and that model-building was premature. His efforts, however, were frustrated by continuing trouble with "Rosy" Franklin. According to Watson's account, Franklin had Wilkins's good crystalline DNA, she was insisting that this research problem was hers, and she would not tell Wilkins her results. Fortunately, they could all learn these results in three weeks when "Rosy" was scheduled to give a seminar on her work. Watson was eager to attend, and he was also inspired to learn some crystallography: "I did not want Rosy to speak over my head." (8)

Watson journeyed to London to attend "Rosy's" seminar on her X-ray crystallographic studies of DNA. In The Double Helix, Watson claimed that he had learned enough in six weeks of conversation with Francis Crick to follow Rosalind Franklin's lecture and to look for details that might be useful for model-building. In her "quick, nervous style that suited the unornamental old lecture hall" and with no "trace of warmth or frivolity," (9) Franklin described her results. Watson observed that clearly she had no interest in building models. "The years of careful, unemotional crystallographic training had left their mark. She had not had the advantage of a rigid Cambridge education only to be so foolish as to misuse it. It was downright obvious to her that the only way to establish the DNA structure was by pure crystallographic approaches." (10)

Anne Sayre's book described this talk in some detail: (11)

What Rosalind reported in her talk [in November 1951] survives in the form of notes which she prepared for the occasion. Because these notes still exist, one can see that they contain very nearly all the material included in the

reports she wrote three months later for Randall. These are documents, not opinions. They inform us that what Rosalind had done was successfully to hydrate fibers of DNA and then to show from the X-ray diffraction photographs which she made that considerable changes took place from the wet to the crystalline to the dry states. The notion of doing this was not original with her; it had occurred to Wilkins; it had been tried before; and according to Gosling, indeed, the X-ray patterns obtained by him before Rosalind came to King's showed enough interesting reflections to indicate that this was a sensible avenue of approach. The difference was that Rosalind applied an improved method of hydration, and then had the advantage, previously lacking, of appropriate X-ray apparatus for the diffraction studies.

Under the influence of effective hydration, there was a change in the fiber which may be described as dramatic. Again according to Gosling, there was a moment when the hydrated fiber literally leaped off the instrument to which it was affixed. . . .

According to Franklin's report: (12)

The highly crystalline fibre diagram given by DNA fibres is obtained only in a certain humidity range, about 70% to 80%. The general characteristics of the diagram suggest that the DNA chains are in a helical form. . . .

During the change "crystalline $\rightarrow$ wet" a considerable increase in length of the fibres occurs. The helix in the wet state is therefore presumably not identical with that of the crystalline state.

. . . With decreasing humidity the crystalline diagram fades away without decreasing appreciably in sharpness. This means that the basic lattice is preserved while disorder about the lattice-points increases, more and more scattered radiation passing from the sharp spots into the diffuse background.

. . . The crystalline state is associated with $\approx$ 20% weight water (in dry DNA.) But both the water content and the structural state of the DNA fibres are highly dependent on history as well as on relative humidity; i.e., there is a strong hysteresis in water uptake.

. . . The fibre diagram of the crystalline form can be indexed tentatively on the basis of a face-centred monoclinic unit cell . . . nearly hexagonal in projection. . . . It therefore suggests that the structure is built up of near-cylindrical units.

. . . The 27A layer-line spacing is very strong, which suggests that it corresponds to one turn of a helix.

. . . Astbury's (13) density measurement . . . together with our water-content measurements, indicate 24 nucleotides per primitive unit cell and 4 molecules of water associated with each nucleotide.

. . . The results suggest a helical structure (which must be very closely packed) containing probably 2, 3 or 4 co-axial nucleic acid chains per helical unit and having the phosphate groups near the outside. It is the phosphate groups which would be capable of forming strong inter-helical bonds in the presence of considerable quantities of water (leading first to the "wet" structure of independent helices with parallel axes, and ultimately to the solution of DNA in water) and would remain strong in the absence of water, thus explaining the cementing effect of strong drying. The dry structure is distorted and strained due to holes left by the removal of water, but contains intact the skeleton of the crystalline structure. (14)

It is a long pull to read this. But it is worth it. Here is the documentary evidence against which opinion and recollection must be measured. In The Double Helix, James D. Watson has provided his recollection and opinion in detail, and they offer a curious contrast. He attended Rosalind's talk and found it, to begin with, lacking in optimism, apparently because she indicated that hard facts would come only when further data had been collected which could allow the crystallographic analyses to be carried to a more refined stage. Whether this proves lack of optimism is debatable; it sounds rather more like a projected program of hard work, something which is usually undertaken in a hopeful spirit, combined with a commendable prudence with respect to unwarranted predicting of results.

He was also troubled that no one brought up the desirability of using molecular models to help solve the structure, and suggests that the general lack of a romantically optimistic response from the listeners, as well as the silence on the topic of models, might have been due to fear of a sharp retort from the woman he inveterately refers to as "Rosy." Romantic optimism, it is true, was not what Rosalind dealt in; scientists are advised to avoid it, as a rule. As for molecular models, it is not entirely clear what mention of them at this point in the progress of the DNA research would have contributed. The dreaded sharp retort from Rosalind on that subject could only have been a statement that she did not see at the moment what basis models could be successfully built upon, and this would have been hard effectively to dispute. Information--hard

facts--that cast light upon the structure of DNA was far from plentiful; Rosalind possessed as much of it as anyone; and that it was as yet insufficient to narrow down the many possibilities for conceivable model structures she knew perfectly well. Watson may have been unaware of the fact that in her graphite work Rosalind had made use of model-building techniques, but being unaware does not seem quite sufficient excuse for an assumption that she cherished categorical objections to the method.

Watson, in fact, did not seem to get a great deal out of Rosalind's talk beyond a depressed feeling that in it he was being told by a woman to refrain from venturing an opinion upon a subject for which he was not trained. But he seems to have gained an impression that he felt worth reporting: "Maurice's mood was . . . jovial. . . . He detailed how, in spite of . . . elaborate crystallographic analysis, little real progress had been made by Rosy since . . . she arrived at King's. Though her X-ray photographs were somewhat sharper than his, she was unable to say anything more positive than he had already. True, she had done some . . . detailed measurements of the water content of her DNA samples, but even here Maurice had doubts about whether she was really measuring what she claimed." (15)

The view is attributed by Watson; it would be entirely unfair to take it literally as Wilkin's own. If Wilkins was skeptical about the accuracy of Rosalind's experiments, or her measurements, the skepticism proved to be unjustified. What Wilkins himself has said is, "I looked at the photograph--that B-form picture--and there it was, you can see the helix right there on the picture, but she refused point-blank to see it. She was definitely antihelical. This is where the real tragedy came in, because if she hadn't had this antihelical attitude, there's no doubt the solution would have been there ahead of Watson and Crick. She was very bloody-minded about it. What could I do? I couldn't even talk to her."

Rosalind's notes for this November lecture say, "Conclusion: Big helix in several chains, phosphates on outside, phosphate-phosphate inter-helical bonds disrupted by water. Phosphate links available to proteins." She underlined it. (The notes are in Rosalind's handwriting, headed, "Colloquium, Nov. 1951." The report is typewritten, dated 7 February 1952, and is corrected in her hand.)

This does not in itself provide the structure of DNA. But it contains some essential clues, without which the structure of DNA could not have been determined at the time at which it was. It is true information and new, more "positive" in that sense than what Wilkins had produced

before Rosalind came or, indeed, than what anyone had produced before her.

It is, of course, incomprehensible that Wilkins could have heard this talk, and yet continued to believe that Rosalind was so "bloody-minded" that she couldn't see the helix in the B-form picture. Nothing could be stronger testimony to the blank wall of noncommunication between them. Some tragedy is always implied when human beings can in no way communicate; what happened at King's is only a special example of it. . . .

It also must be noted that Rosalind's affection for "hard facts"--a liking not uncommon in scientists and not often reproved--was, in this case, perfectly sensible, and just as sensible from the viewpoint of model-building as from any other. Models are not built out of thin air or random inspiration; they do not come into being quite as magically as Watson appears to suggest. What model-building consists of, briefly stated, is the successive hypothesizing of molecular models until one is arrived at which has its atoms arranged in such a way that the diffraction pattern--and everything else--is accounted for, never a really simple thing to do, and probably in all instances something which demands a certain flair. But it also demands a very extensive supply of information, of hard facts; if this is lacking, a model-builder is less likely to produce a molecular model than a castle in the air, a wish fulfillment.

The data required, the hard facts, are immense. In their absence no plausible model can be built, none can be verified. The arrangement of atoms arrived at as a proposed structure for any molecule must account satisfactorily for the diffraction pattern; embrace without contradiction all known chemical, physical, crystallographic--and in some cases, biological--information; and manage to defy no law of nature. Quite plainly, if nothing is known about a substance, a model cannot be built at all; if too little is known, then any model produced will be vague, uncertain, composed chiefly of hope, and insufficiently verifiable.

How much information is needed? In Rosalind's opinion, a good deal more than existed in the autumn of 1951. She had no prejudices against model building; it was a technique which she had used to a degree in some of her carbon work in Paris, and one which she was subsequently to use in her work on viruses. She was not dedicated to it, however; and she could not have helped being aware that at Cambridge, where model building was practiced with much enthusiasm, there had been one notable failure of the method as well as a number of successes. Certainly she knew that the model-builder who has not got enough to go on may well

come up with an attractive, pleasing, sensible-looking proposition which is all the same unprovable, and which may even be quite wrong. Linus Pauling proposed an incorrect model for DNA. At the end of 1951, so did Watson and Crick.

The day after Rosalind Franklin's seminar, Watson tried to tell Francis Crick about the talk. Crick himself had not attended the seminar, because it would have been unfair competition; Maurice Wilkins should have the first chance to apply "Rosy's" data to his research problem. In The Double Helix Watson reports that his account of the seminar was annoyingly vague. He did not know enough of the terminology, and he did not remember such specific details as the water content of the DNA that Franklin had measured. He also failed to notice Franklin's conclusion that the phosphate groups are located on the outside of the molecule. Nonetheless, Watson and Crick proceeded with their efforts to construct a model.

In constructing their first three-dimensional model of the DNA molecule, Watson and Crick assumed that the nucleotide chains were arranged with the sugar-phosphate groups in the center of the helix, held together by a divalent ion such as magnesium.

After fiddling with bits of metal supported awkwardly by pinchers, they produced their model: three sugar-phosphate chains twisted around each other, with a distance between coils that fit the crystallographic data.

Sayre reported:

According to The Double Helix, they were rather pleased with their product, though the structure they proposed was disappointingly unilluminating. Wilkins was invited to Cambridge to witness the triumph; William Seeds, who worked with Wilkins, came along, and Rosalind and Gosling as well. The session was opened by Crick with an exposition of helical diffraction theory, a subject upon which he was very expert, and went on to a description of the model, of which Rosalind plainly did not think much. Her disdain of it Watson accounts for on the grounds that what was proposed was a helical structure, while Rosalind did not admit that a shred of evidence existed to indicate that DNA was helical--a curious statement, considering that very shortly before she had presented a good deal of evidence suggesting that the B form of DNA was exactly that. What she did object to in the proposed structure--and aggressively, we are told--was that the three-chain model had its phosphate groups held together by Mg^{++} ions in a way she thought unlikely, considering that by her calculations the Mg^{++} ions would be surrounded by tight shells of water molecules.

And as Watson was required to confess, her objections, though very annoying, were not mere perversity.

In fact, Watson admitted, "we were up the creek with models based on sugar-phosphate cores. No matter how we looked at them, they smelled bad." (16) Further fiddling did not help. Enough was known about chemical bonds and atomic spacing to set some limits on how close one atom could be to its partners, and these limits had to be observed when the atoms were arranged in space. Either new data or new ideas were necessary before more model-building could be attempted.

Interlude and More Work

Following this failure, Crick resumed work on his Ph.D. thesis, and Watson began a study of tobacco mosaic virus. The story provides another opportunity to compare the working conditions and methods of Watson and Franklin.

As noted previously, Franklin had several years of experience in crystallography and physical chemistry, and she worked long and hard to set up her X-ray camera in her laboratory in London. Watson learned enough theory from Crick to interpret X-ray photographs. Another scientist, Hugh Huxley, showed Watson how to use the X-ray apparatus that was set up at the Cavendish Laboratory, and Watson obtained some virus for his study from still another scientist. In approximately six months Watson had an X-ray photograph good enough to show that the proteins of the virus were helically arranged. The nucleic acid component of the virus, however, was too implicated for a rapid analysis, especially by someone relatively unskilled. Thus it seemed appropriate to return to the DNA problem. The news from Franklin and Wilkin's London laboratories was not encouraging. Neither Franklin nor Wilkins was building models, and "the squabbling between Maurice and Rosy was more bitter than before." (17) Furthermore, "she was insisting that her data told her DNA was not a helix. Rather than build helical models at Maurice's command, she might twist the copper-wire models about his neck." (18)

New Competition

In mid-January 1953, Watson and Crick were running in pursuit of DNA again, spurred on by a manuscript by Linus Pauling. The manuscript came into their hands from Pauling's son Peter, who was a research student at Cambridge. Pauling's model consisted of a three-chain helix with the sugar-phosphate backbone in the center. The chains were held together by hydrogen bonds formed by hydrogen atoms bound to the phosphate groups. Watson and Crick's understanding that the phosphate groups were negatively charged and the hydrogens not bound was confirmed by chemists in nearby laboratories. To their relief, Pauling's model was wrong. Maurice Wilkins had to be informed about this competition from the Americans, and so Watson took the manuscript to Wilkins's laboratory.

According to Watson, he walked unannounced into "Rosy's" laboratory and showed her Pauling's manuscript. He explained Pauling's mistake and also mentioned for her amusement the similarities between Pauling's model and the model he and Crick had devised fifteen months previously. Watson stated that "Rosy became increasingly annoyed with my [Watson's] recurring references to helical structure. Coolly she pointed out that not a shred of evidence permitted Linus [Pauling], or anyone else, to postulate a helical structure for DNA." (19) Watson interrupted her "harangue" to defend the idea of a helix. "Rosy by then was hardly able to control her temper." She told Watson not to be so stupid and to look at her evidence. Since Watson had heard about her "so-called antihelical results" from Wilkins, and since Crick had told him the interpretation was wrong, Watson decided to assert that Franklin was "incompetent in interpreting X-ray pictures" and ignorant of the necessary theory. In response Franklin suddenly moved toward Watson, who grabbed the manuscript and fled in fear that "in her hot anger she might strike" him. (20)

Watson was saved by the entrance of Wilkins, and the two escaped together. Wilkins was not surprised by Watson's account of the encounter and said the same thing had happened to him. He and Franklin had almost come to blows following an argument in his room." (21) As they retreated from their mutual enemy, a new comradeship developed. Watson now knew by experience the "emotional hell" Wilkins has endured the past two years. Wilkins then confided that he and an assistant had been doing X-ray work with DNA and were prepared for an all-out effort after Franklin departed for another laboratory.

New Data

Wilkins also told Watson about some new results that Franklin had obtained. The A form of DNA, which Franklin had studied earlier, gave a complex X-ray diffraction pattern, and the helical symmetry was difficult to discern (hence Franklin's doubts about the helical nature of DNA). The new, B form contained more water and gave a much simpler pattern. A glance at Franklin's picture showed Watson that the B form was clearly helical, and important dimensions could be calculated easily. The data necessary for a new assault on the structure of DNA were in Watson's hands.

In this conversation with Wilkins in London, Watson ascertained that Wilkins was not convinced that the DNA structure was helical. Wilkins was also convinced that Franklin was correct in her conclusion that the sugar-phosphate backbone was on the outside of the helix. Wilkins's colleague, R. D. B. Frase, was trying to develop a three-chain model, but the real problem was packing the bases in the center of the helix, and for this problem they did not yet have a solution.

Another Attempt at Model-Building

Watson returned to Cambridge with the new data about the dimensions of the helix. He did not believe Franklin's conclusions that the sugar-phosphate backbone lay on the outside, nor did he have good

grounds for deciding between two or three chains per molecule. For no apparent reason other than that "important biological objects come in pairs," Watson decided to build two-chain models. He had a machinist start making the necessary metal models of the organic bases and the phosphorus atoms. The phosphorous atoms came quickly, and he assembled short chains of sugar-phosphate groups. He worked to arrange these groups in a two-chain model with the sugar-phosphates in the center, but no satisfactory fit was attained.

The organic bases were the real problem. If they were inside, how could their irregular shapes be stacked to give a regular structure? There might be a large number of possible ways, with no means for deciding which was correct. However, the bases could be ignored for a while, since it would take the machine shop another week to manufacture them.

Because the backbone-inside models were ugly and objectionable, Watson began to fiddle with backbone-outside arrangements. When this was done, the model backbone fitted easily into a shape compatible with the X-ray data. The data showed a repeating structure every 34 Å, and this structure could be represented by a 34 Å spacing between turns of the helix. The arrangement of the bases, however, was still a problem. According to The Double Helix, Crick continued to work on his thesis problem, and Watson played tennis, had sherry with the girls, and went to the movies while he waited for a new idea.

Communication of Data

Watson obtained some of the X-ray data that he used in his first backbone-outside model from a report prepared by Rosalind Franklin and other scientists at the King's College, London, research center. This report had been submitted by J. T. Randall, who was head of the research center, to a government committee. Watson saw the report because of his acquaintance with a member of that committee and unbeknownst to Rosalind Franklin or anyone else in the London research group. Anne Sayre described the report and the events surrounding it as follows:

In December 1952, only two months before Watson paid his February visit to King's, the biophysics committee of the Medical Research Council held a meeting. The meeting was at King's College, for Randall was a member of the committee and on that occasion was its host. At the meeting, Randall circulated a report covering recent work done in the laboratory under his direction--this was a matter of course--and the report included a concise summary, signed by Rosalind and Gosling, of the results of their X-ray studies of calf thymus DNA. Now, among those present was another committee member, Max Perutz, a very distinguished member of the Cavendish Laboratory at Cambridge. In the natural course of events, Perutz received a copy of the report. About this Perutz has written,

As far as I can remember, Crick heard about the existence of the report from Wilkins, with whom he had frequent contact, and either he or Watson asked me if they could see it. I realized later that, as a matter of courtesy, I should have asked Randall for permission to show it to Watson and Crick, but in 1953 I was inexperienced and casual in administrative matters, and since the report was not confidential, I saw no reason for withholding it. (22)

Nor was there any reason for withholding it, not, at least, in any normal situation. Watson writes about this in a quite different vein. At this time he and Crick had had one of the great insights which led them to the structure of DNA, and he was very elated, as he had every reason to be, to be assured that what they had in mind was not incompatible with the experimental data. By experimental data, he meant Rosalind's. As he admits, neither Rosalind nor anyone else at King's had directly provided this information, and none of them realized what Crick and Watson had in their hands. The data had been provided because Max Perutz was a member of the Medical Research Council's biophysics committee, which Watson describes as "a committee appointed . . . to look into the research activities of Randall's lab. Since Randall wished to convince the outside committee that he had a productive research group, he had instructed his people to draw up a comprehensive summary of their accomplishments. . . . As soon as Max saw the sections by Rosy and Maurice, he brought the report . . . to Francis and me."

There are several strands here to be sorted out. The suggestion that the committee was a special one set up to examine the productivity of Randall's laboratory is, according to Perutz, inaccurate. A committee set up in 1947 to "advise regarding the scheme of research in biophysics under the direction of Professor J. T. Randall" was dissolved before the end of 1947. The committee which existed in 1952 was formed "to advise and assist the Medical Research Council in promoting research work over the whole field of biophysics in relation to medicine." (23) What is significant is that once again Watson has taken the opportunity to suggest that such chaos or inadequacy prevailed at King's College that it had removed itself from serious consideration as a place at which science was done. Another strand is the exact part played by Max Perutz in the transmission of the information. When The Double Helix appeared, Perutz was understandably distressed by Watson's description of his action. He has pointed out dryly that the report he showed to Crick and Watson contained, after all, no data that "Watson had not already heard from Miss Franklin and Wilkins themselves. It did contain one important piece of crystallographic information useful to

Crick; however, Crick might have had this more than a year earlier if Watson had taken notes at a seminar given by Miss Franklin." (24)

Though Watson subsequently denied, in a reply to Perutz's published rebuke, that "the King's lab was generally open with all their data," (25) Perutz had a comment upon this too: "It is interesting that a drawing of the 'B' patterns from squid sperm* is also contained in a letter from Wilkins to Crick written before Christmas 1952. . . . This clearly shows that Wilkins disclosed many, even though perhaps not all, of the data obtained at King's to either Watson or Crick." (26)

Whether a laboratory is under any obligation to be generally open with data is an arguable point. Certainly there are circumstances in which reasonable prudence would advise it not to be. In this case it is evident that what was being done at King's was communicated to Cambridge in a variety of ways, some open and some at best accidental.

The question is, of course, whether any of this made a difference. The answer is plainly that it did. The difference was that of time. It appears that Crick and Watson had, on February 5, 1963, nothing in mind which permitted them then to build a conclusive model of DNA, but between February 6 and February 28, a successful model had become possible. Most certainly the data received from King's were not all that was required to allow them to do this, but equally certainly they were essential. Not only did Rosalind's density data indicate the possibility of a two-chain model, but the diffraction pattern she obtained in the B-form photographs provided evidence of the diameter of the helix. More than this, "Rosy had hit it right in wanting the bases in the center and the backbone outside," a configuration to which Watson had been, for his own part, rather resistant.

None of this was information which Crick or Watson had developed through independent experimentation. No doubt it would have been accessible to them if they had tried. But such experimenting would have been, at the very least, time consuming; and the more so in Watson's case, as he possessed at the time little experience in the methods by which it was done. This means, if nothing else, that the model based upon independently gathered experimental information could not have seen the light of day by the end of February 1953.

*The work on squid sperm was done by Wilkins himself.

The Puzzle of the Bases

Given the dimensions of the helix and the backbone-outside configuration, the next problem was the arrangement of the bases. Watson needed a clue, and he repeatedly turned over in his mind the information that he had. To look for a fit between the bases, he drew their structures (Figure 2), taking the formulas from a book on nucleic acids. The four bases had different shapes and sizes, and there seemed to be no way to fit them inside a regular helix with uniform dimensions. The available evidence suggested that there was no particular order to the distribution of the bases along the backbone. Hence randomly coiling two backbones around each other would be expected to result in collisions between the larger bases and weak spaces between the smaller bases. Furthermore, the forces holding the two chains together were still a problem, although Watson had recently come to believe, on the basis of both chemical and X-ray crystallographic analysis, that hydrogen bonds were formed between the bases.

Base-pairing, Like with Like

After making futile sketches for more than a week, Watson suddenly got an inspiration. Suppose adenine in the DNA formed hydrogen bonds as it did in crystals of pure adenine-- adenine (A) bound to adenine. Similarly the other organic bases could form like pairs: guanine (G) to guanine, cytosine (C) to cytosine, and thymine (T) to thymine (Figure 3). There was the problem that the A-A pairs and the G-G pairs were bigger than C-C and T-T pairs, and thus did not fit a uniform backbone, but perhaps there were "minor in-and-out buckles" to accommodate these differences.

Watson recalled, "despite the messy backbone, my pulse began to race." (27) For what the model offered was an explanation of how DNA--and genes--replicated. A new chain was formed on the old, by pairing of like bases. However, after the initial excitement wore off, some disturbing thoughts entered. What was to keep adenine from pairing with guanine? The accepted structures of the organic bases would allow several other erroneous pairs to form. This problem was dismissed with the suggestion that perhaps some specific enzyme was involved that recognized only like molecules. Watson said, "As the clock went past midnight I was becoming more and more pleased. . . . For over two hours I happily lay awake with pairs of adenine residues whirling in front of my closed eyes. Only for brief moments did the fear shoot through me that an idea this good could be wrong." (28)

"My scheme was torn to shreds by the following noon," Watson reported. (29) Hydrogen ions can shift their position (Figure 4), and Watson had chosen the wrong molecular arrangement for thymine and guanine. A visiting American crystallographer, Jerry Donohue, persuaded Watson that the usual textbook pictures were incorrect, although he admitted that the data were not conclusive. Watson accepted the new arrangements reluctantly, but he respected Donohue's expertise. The new shapes made the size differences between G-G and T-T even bigger, and

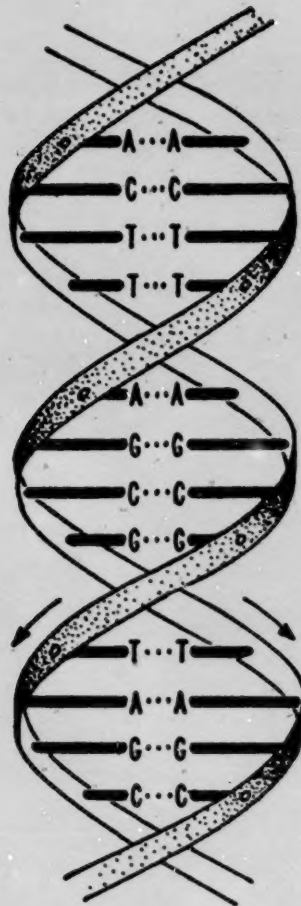


Figure 3. A double helix with pairing between like bases, adenine to adenine (A), cytosine to cytosine (C), thymine to thymine (T), and guanine to guanine (G) (redrawn from The Double Helix).



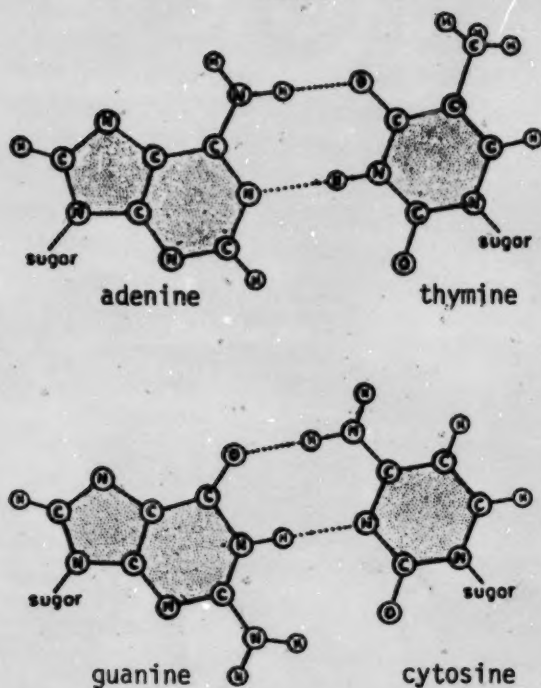


Figure 5. Hydrogen-bonding between unlike bases (redrawn from The Double Helix).

A New Model: A-T and C-G Pairs

The pairing of A and T, and G and C, was an important discovery. As a result of this arrangement, there were no size discrepancies between the base pairs, and they could be arranged in an irregular sequence along a regular backbone (Figure 6). The new model explained Chargaff's data showing that the number of G plus A molecules equaled the number of C plus T molecules in purified DNA samples. More exciting was the implication for replication of the DNA. Because the pairs of bases that could form are restricted, a single chain acting as a template can determine the sequence of bases in a new chain that forms.

Watson and Crick did not know yet whether the newly devised base-pairs would fit nicely into the backbone configuration erected during the preceding two weeks. Although they were anxious to proceed, in order to save the geneticists and biochemists some time and effort and to win the race with Pauling, an acceptably accurate three-dimensional model could not be contracted without the metal molecular models of the bases. Watson's cardboard cutouts were too imprecise and flimsy.

The next day the machine shop produced the necessary metal plates, and a model was quickly constructed. It satisfied the X-ray data and the laws of stereochemistry that set limits on interatomic spacings. The model, the spring sky, and the world looked very good.

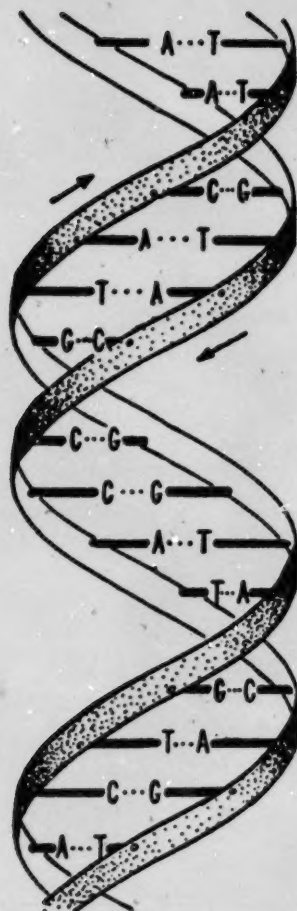


Figure 6. A double helix with hydrogen bonds between adenine and thymine and between cytosine and guanine (redrawn from The Double Helix).

Checking the New Model

This time however, they were cautious about revealing their model without carefully checking the exact coordinates for all the atoms. The idea and especially its implications for DNA replication were so good that their enthusiasm might have led them astray. Since the components of the model and the bond lengths were made to scale, they measured the model with plumb line and ruler to ascertain the three-dimensional coordinates of all the atoms in a nucleotide. Everything fit, although they could not do the final checking without the exact X-ray evidence, which they did not have.

Maurice Wilkins came from London to have a look at the model. He liked it. He accepted the fact that it had two chains, not three, since he knew the evidence on that point was ambiguous. He accepted without question the configuration of guanine and thymine that Jerry Donohue had proposed. He did not mention their good fortune in having Donohue's advice that all the structures pictured in textbooks were misleading. He returned to London to make the measurements on the X-ray photographs so that the model could be carefully evaluated. Two days later Wilkins reported that both he and Franklin found that their data supported the model. Wilkins and his collaborators, as well as Franklin and her student, Gosling, wanted to publish their data simultaneously with Watson and Crick's theoretical paper.

Publication of the Results

Watson and Crick published their model in a short paper in Nature, April 25, 1953. Sayre commented:

The structure which they proposed for DNA is beautiful and true. It has a natural simplicity which convinces; it answers questions which have proved it from the beginning to be the uniquely correct answer to an immensely difficult puzzle; it has gone on answering enough more questions to make it the basis of a new field of science. Nothing has arisen in the course of subsequent work which has in any way contradicted it, and nothing is ever likely to, for all subsequent work has unfailingly confirmed and reconfirmed it.

That what Watson and Crick did in order to come up with their brilliant perception was to put together many fragments of pre-existing information is no argument against their accomplishment, for this is what science consists of and, in any case, no form of creativity should ever be assessed in terms of a plunge into the black waters of total ignorance in the hope of finding a possible oyster that may or may not contain a pearl. To rearrange a universe is creativity enough for anyone short of God, and this is very close to what they did, something within the capacity of few indeed.

This is enough, and more than enough. There is no need to suggest that it was also less a perception on the part of two scientists than a mystical revelation. How this notion crept in is clear; there is also some reason to guess that one motive which might have moved Watson to write The Double Helix and confess that revelation had little to do with it was a desire at last to put the notion to rest. But what had given rise to it from the outset was the paper that Watson and Crick published in Nature on April 25, 1953--"A Structure for Deoxyribose Nucleic Acid"--which, for all the stunning content and the exquisite style, has been fairly characterized as being only a little longer than the Ten Commandments and rather similar in its quality of appearing to have been handed down by God to the prophets waiting on Mount Sinai. . . . Watson and Crick's Nature paper, less than a thousand words long, comes as close as is possible to suggesting pure perception, missing by very little. But quite apart from the natural temptation to present it this way, they were also, no doubt, somewhat inhibited by circumstances. What came their way from King's College, having come their way unorthodoxly, could not be credited in an orthodox way, and this explains a paragraph otherwise a little baffling, and perhaps somewhat elusive:

The previously published X-ray data on deoxyribose nucleic acid are insufficient for a rigorous test of our structure. (30) So far as we can tell, it is roughly compatible with the experimental data, but it must be regarded as unproved until it has been checked against more exact results. Some of these are given in the following communications. We were not aware of the details of the results presented there when we devised our structure, which rests mainly though not entirely on published experimental data and stereochemical arguments. (31)

The "following communications" are two supporting papers which appeared in the same issue of Nature, April 25, 1953, with the Watson-Crick paper that proposed the DNA structure, all three appearing under the modest general heading, Molecular Structure of Nucleic Acids. These supporting papers were "Molecular Structure of Deoxypentose Nucleic Acids," signed by M.H.F. Wilkins, A.R. Stokes, and H.R. Wilson, and "Molecular Configuration in Sodium Thymonucleate," by Rosalind E. Franklin and R.G. Gosling. Supporting papers are what they are, offering experimental evidence which confirms the Watson-Crick structure; and doubtless they were not seen at Cambridge before the structure paper was written. What was known there, however, was a good deal of unpublished data accumulated at King's, and chiefly by Rosalind, which is

dimly acknowledged, if acknowledgment can be read at all into the statement that "our structure . . . rests mainly though not entirely on published experimental data and stereochemical arguments."

What Crick and Watson did was to put together in a way which no one had seen before several isolated pieces of information about DNA.

According to an account by Olby, quoted by Sayre (32):

Evidence so far collected suggests that this successful attempt in 1953 to determine the structure of DNA took from Friday, February 6, when Watson took Pauling's DNA manuscript with him to King's College, London, until Saturday, February 28, when Crick retired to bed exhausted after nearly a week of model-building. At King's, Watson learned from Wilkins that the density data did not after all rule out two-chain models and that the sugar-phosphate chains must, as Franklin had stated in Watson's presence in 1951, be on the outside. At the end of the first week back in Cambridge, Watson had come round to this view. Crick recalls an earlier incident when Watson was complaining of the difficulties he was having in the attempt to intertwine the backbones on the inside of the structure. Crick's reaction was to ask, "Then why don't you put them on the outside?" Watson replied: "That would be too easy," which prompted the retort: "Then why don't you do it!" Watson himself wrote: "Finally . . . I admitted that my reluctance to place the bases inside partially arose from the suspicion that it would be possible to build an almost infinite number of models of this type. Then we would have the impossible task of deciding which one was right. But the real stumbling block was the bases. As long as they were outside, we did not have to consider them. If they were pushed inside, the frightful problem existed of how to pack together two or more chains with irregular sequence of bases. Here Francis had to admit that he saw not the slightest ray of light. (33)

From this, two things at least are evident. One is that model-building, as an approach to structure determination, is not limitless in its powers. Unless it incorporates a great deal of defining information, it can easily produce an almost endless number of unprovable and untestable hypotheses. Quite evidently Crick and Watson had come so far but no further. From this the second point follows. Without the information acquired by Watson on his visit to King's, the successful model of DNA would not have been completed at Cambridge by February 28, or indeed, any date close to that.

For this contribution of time, Watson and Crick offered thanks in their paper. "We have also," they said, "been stimulated by a knowledge of the general nature of the unpublished experimental results and ideas of Dr. M.H.F. Wilkins, Dr. R.E. Franklin, and their co-workers at King's College, London." (34)

"Stimulated" seems a minimal word for it.

On February 28, then, Crick and Watson completed their model. They were charmed with it, as well they should have been. By every test they could apply, it "worked." It did more than that. As they put it, in the most delightful throwaway line ever incorporated into a scientific paper, "It has not escaped our notice that the specific pairing we have postulated immediately suggests a possible copying mechanism for the genetic material." (35) Their structure explained.

There was, of course, no need for them to hesitate about publishing this beautiful thing. What there might have been some hesitation about was the form the publication should take. An offer might have been made, in view of everything, to the people at King's for joint publication, but that this possibility existed seems to have crossed no one's mind. But that Wilkins, at least, was not immediately informed seems perhaps a little surprising. That Watson recalls having mused over the fact that Maurice must soon be told indicates that he, if no one else, caused some conscientious qualms. Letters promptly went out to places further away than London, which can be reached from Cambridge by telephone quite inexpensively, but Wilkins apparently was left to learn of the triumphant joy at the Cavendish until the paper was written. Olby says,

On that Wednesday [March 18] Wilkins received the Watson-Crick typescript, or perhaps several versions of it, for on the 18th he wrote:

"Thanks for the MS. I was a bit peeved because I was convinced that the 1:1 purine-pyrimidine ratio was significant," and adds, "Just heard this moment of a new entrant in the helical rat-race. It seems that they (R.F. and G.) should publish something too. (They have it all written.) . . ."

The only construction that I can put on Wilkins's letter is that the first he knew of the structure was when he got these MSS. Yet Watson writes in his book that when the model had been completed he told Francis he would write that afternoon to Luria and Delbrück: "It was also arranged that John Kendrew would call up Maurice to say he should

come out to see what Francis and I had devised!" Yet Watson's letter to Delbrück is dated March 12th. Franklin and Gosling wrote their draft on or before the 17th, and Wilkins first learnt about the double helix on March 18th. (36)

No doubt elation, or embarrassment, or some combination of the two, might explain why Wilkins heard about the triumph of his Cambridge friends only just in time to be able to contribute a supporting paper to the same issue of Nature in which the triumph was announced. The question of joint publication never arose. Nor, of course, did it arise with respect to Rosalind.

That she and Gosling had a paper all written in advance of March 17 is in itself interesting. Clearly her own work had reached a point, quite independently of anything that might be going on at Cambridge, where she felt that something worth publishing had been achieved. It has been natural to assume that the paper by her and Gosling which appeared in the April 25 issue of Nature--"Molecular Configuration in Sodium Thymonucleate"--is the same as the one prepared before the news from Cambridge arrived. This paper discussed both the A and B forms of DNA--including the X-ray photograph of the B form with its helical cross--and, in addition to affirming in published form the location of the phosphate groups on the outside of the molecule, opts for a two-chain helix in the B form and probably in the A form as well.

In 1969, Aaron Klug discovered among the papers Rosalind left at her death a manuscript in draft, dated March 17, 1953, which clearly is what Wilkins was referring to as the paper "all written," and which, though a precursor of the Nature paper, is not identical to it. This also proposes that the B form is helical, that the diameter of the helix is about 20 Å, and that a crystallographically "very important part of the molecule lies on a helix of this diameter." This can only be the phosphate groups. . . . Thus, if the structure is helical, we find that the phosphate groups lie on a helix of a diameter about 20 Å, and the sugar and base groups must accordingly be turned inwards towards the helical axis." And from there she proposes two equally spaced coaxial helices.

She came very much closer to the discovery of the double helix than she has usually been credited with doing, and the date on the draft paper makes quite clear when she did so. The existence of this paper does not challenge for a moment the priority of Crick and Watson's structure; but it does make nonsense of a great deal which has gotten into print, some of it by Watson. The discovery of the draft

paper was communicated by Klug to Watson, who responded with amazement, suggesting that perhaps Rosalind had been inspired by events in Cambridge, passed along to her by Wilkins. But such inspiration was not necessary. Klug had previously studied Rosalind's notebooks preparatory to writing an article about her contribution to the DNA discovery, (37) and was able to confirm from them the direction in which her thinking was moving well before she wrote the draft paper of March 17.

She was close; she was very close; the pieces were beginning to come together in the right way, and it should be remembered that when the pieces begin to fall in place, they often fall quite fast. It should also be emphasized that Rosalind was working essentially alone. The contrast between what was available to Crick and Watson in Cambridge and what Rosalind had for stimulation and support at King's is a staggering one. The perception of the significance of Chargaff's work in providing the 1:1 ratio was a splendid one, but it was assisted by a useful conversation with a mathematician. How long would Watson have wrestled with the problem of the bases had Donohue not been present to suggest the keto instead of the enol forms? Watson had Crick, and Crick had Watson. Rosalind's available source for conversation, advice, [and] helpful speculation was Luzzati, in Paris. Because she had done all her previous crystallographic work in France, she was not even acquainted at the time with the people in England who were working in the field. At King's she had Gosling to talk to and--to a limited degree--Randall. . . .

One indication of her nearness to the solution of the problem was her instant appreciation of the rightness of the Watson-Crick model. She amazed Watson, or so he says, by producing no irrelevancies to cast doubt upon the correctness of the double helix. His fears about the failings of "her sharp, stubborn mind, caught in her self-made antihelical trap" (38) proved to be illusory, very likely because the antihelical trap was of Watson's devising, though he prefers to think it was because X-ray evidence itself had begun "forcing her more than she cared to admit" (39) in the direction of accepting a helical form. The meeting with Rosalind at which the Cambridge model was displayed to the King's College group turned out not to be as dreadful as Watson had anticipated, but he had had warning of a change in her attitude from a visit paid to King's by Crick, at which Rosalind appeared ready to "exchange unconcealed hostility for conversation between equals." (40) When Rosalind demonstrated to Crick how "foolproof was her assertion that the sugar-phosphate backbone was on the outside of the molecule," Watson subsequently reflected upon this in a curious vein: "Her past uncompromising

statements on this matter thus reflected first-rate science, not the outpourings of a misguided feminist." (41)

The notion that accurate statements made by a woman scientist are first to be regarded as likely outpourings of feminism, and only under the strong pressure of irrefutable demonstration as science is Watson's own contribution. So is the fictional "Rosy." Unfortunately these Watsonisms have been widely believed. From scientific publications to discussions in the popular press, both Rosalind's views and her contributions to the solution of the structure of DNA, and sometimes her personality as well, have been either misrepresented or overlooked.

Footnotes

1. James D. Watson, The Double Helix (New York: New American Library, 1969).
2. Anne Sayre, Rosalind Franklin and DNA (New York: W. W. Norton & Co., 1975).
3. Rosalind Franklin, Interim Annual Report (1 January 1951 - 1 January 1952).
4. Ruth Hubbard, "Rosalind Franklin and DNA." In Signs 2 (Autumn 1976): 229-237.
5. Watson, The Double Helix, p. 19.
6. Ibid., p. 20.
7. Ibid.
8. Ibid., p. 43.
9. Ibid., p. 15.
10. Ibid., p. 51.
11. Sayre, pp. 123 ff.
12. Hubbard, pp. 229-237.
13. Sayre's note: W. T. Astbury was among the earliest workers on DNA. He obtained the first X-ray diffraction pictures of the substance.
14. Franklin.
15. Watson, The Double Helix.

16. Ibid., p. 69.

17. Ibid., p. 81.

18. Ibid.

19. Ibid., p. 105.

20. Sayre commented on Watson's account of this incident:

Wilkins says, "Jim somewhat exaggerated. . . .I can imagine that she might have slapped someone's face, but that's not physical attack. Jim wrote a novel. "

Indeed, it does seem rather exaggerated that any man would fear assault by a small, slim woman who did not even know karate. People who knew Rosalind incline to find Watson's account of this dangerous moment merely absurd, though not from his point of view necessarily unpurposeful.

21. Watson, The Double Helix, p. 106.

22. Max Perutz, Science 164 (1969), 1537-1538.

23. Ibid.

24. Ibid.

25. James D. Watson, Science 164 (1969), 1539.

26. Perutz.

27. Watson, The Double Helix, p. 118.

28. Ibid.

29. Ibid., p. 120.

30. Here the following are cited: Astbury, W.T. Symp. Soc. Exp. Biol. 1, Nucleic Acids, 66 (Camb. Univ. Press, 1947). Wilkins, M.H.F., and Randall, J.T. Biochim. et Biophys. Acta 10, 192 (1953).

31. Hubbard.

32. Robert Olby, "Francis Crick, DNA, and the Central Dogma," Daedalus 99(4): 958.

33. Ibid.

34. James D. Watson and Francis H. C. Crick, "A Structure for Deoxyribose Nucleic Acid," Nature, 1953: 737.

35. Ibid.

36. From a letter by Robert Olby to Aaron Klug (cited by Sayre).
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40. Ibid.
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Commentary and Questions for Discussion

The story of the elucidation of the structure of DNA illustrates particularly well several facets of the process of discovery. Consider the following aspects:

1. Development or improvement of a technique may make necessary data available, and with new data the probability of successfully solving a problem increases. For example, improved methods of X-ray crystallography gave additional and more precise data about the structure of DNA.

2. The process of discovery involves many steps. Progress may be slow, and some efforts fail. What correct steps were taken by Watson and by Franklin? What false steps were taken, and why did each err?

3. Crucial facts may be available, but not used until relatively late in the sequence of events. One example is Chargaff's data on A-T and C-G ratios. Are there other examples?

4. In the process of discovery, data from many sources are integrated into a whole. What data were available to Watson and Crick, and how were the data used to form a coherent picture of the DNA molecule?

5. With one key insight, many pieces of information fall into place. What was the key insight in the DNA story? Is it valid to say that there was only one key insight?

6. Chance events may influence the direction and rate of progress. Chance encounters with the right people are one example. What are some specific examples from the DNA story? How does the idea of chance events fit with the usual picture of the scientific method?

7. Model-building, either the construction of physical models or the elaboration of mathematical, computerized, or mental models, corresponds to the development of an hypothesis, which is then tested against the available data. How is this generalization illustrated by the DNA story? Consider the functions of data-gathering and model-building in scientific

work. Is there a usual chronological sequence in which the two approaches are employed? Can a scientist use one approach and not the other? Which contributes more to scientific advance?

In addition to the impersonal aspects listed above, personalities can have an effect on how science is done. Consider the adherence to preferred methods. Watson was ready to begin model-building, while Franklin thought that more data were required. What are the relative roles of reason and intuition in the choice of a method?

Communication among scientists is an important aspect of the process of discovery, and it also can be influenced by personalities. As the DNA story illustrates, communication may depend on the printed word, or it may involve ephemeral conversations. Published results and ideas are equally accessible to all. Personal conversations are not. They are dependent in part on circumstances and personalities. If an individual is working in an active laboratory at a well-known and respected institution, the probability of productive encounters is increased. If an individual is relatively high in the scientific hierarchy (discussed in the next subsection) or has access to such high-status individuals, he or she can learn more quickly and easily about new techniques and ideas. If an individual is assertive and ready to seize or create the appropriate circumstances and opportunities, then personal contacts develop, channels of communication open, and future personal and scientific progress is facilitated.

Informal communication between colleagues is well illustrated by the example of Watson and Crick. What information and ideas did they exchange, and how important was their collaboration to their scientific development? For example, consider the instruction that Watson received from Crick on X-ray crystallography. In the absence of such tutoring from an experienced scientist, the novice in a field may waste many days and months developing the understanding that is necessary for using a technique or interpreting data but that is not conveyed by terse scientific publications.

The constructive exchanges of ideas and information between Watson and Crick and also Wilkins contrast strongly with the relative isolation in which Rosalind Franklin worked while at King's College, London. Franklin's previous environment in Paris had been very different and rich in camaraderie, as Sayre has described. In general, there can be a failure of communication between two male scientists or two female scientists, as well as between a male and a female scientist. One or both can be at fault, depending on personalities, demands on time, and other pressures. Additional difficulties may arise between male and female scientists, however, because of differences in socialization as

well as in language and behavior. (Language is discussed in the last article in this section.) How do you explain the difference between the interactions of Wilkins and Franklin compared with the interaction between Watson and Crick?

Various motivations of scientists are also well illustrated by the DNA story. What were Watson's motives? What does Watson's behavior suggest about his motivation? (Consult The Double Helix for more information on these questions.) Consider the same questions for Rosalind Franklin.

Although motivations may differ, a generally accepted code of ethics governs the behavior of scientists. A clear and important principle that is rarely transgressed is truthfulness in the reporting of methods and results. Other standards are less clear, and troubling questions arise. Under what circumstances should one refrain from working on a scientific puzzle that another has begun? When is it ethical to put one's ideas and logic to work on that puzzle? How much use may one make of another's unpublished data and ideas, especially when it is not clear who first thought of the good idea? How readily should a scientist share his or her data? What answers to these questions are suggested by the DNA story?

In addition to illustrating issues pertinent to the process of scientific discovery, additional issues related to the history of science are raised by the DNA story. One of these issues concerns the personality and behavior of Rosalind Franklin. Compare Watson's descriptions of "Rosy" with Sayre's descriptions of Rosalind. Which image is correct, and how can you tell? Compare the methods used by historians and biographers with the methods used by scientists. Are scientists less biased and more objective than other scholars? Another issue concerns the assigning of credit for scientific contributions. What was Rosalind Franklin's role in the elucidation of the structure of DNA? On what basis can her contribution be assessed? Has she received proper credit for her work? Rosalind Franklin died of cancer in 1958 at the age of 37. Watson, Crick, and Wilkins shared the Nobel prize in 1962 for their elucidation of the structure of DNA.

Status and Rewards

Scientists may receive three kinds of rewards for their work: (1) the psychological benefits of recognition and prestige within the scientific community, (2) the practical benefits of employment and a decent salary, and (3) the personal satisfaction of doing something enjoyable and challenging.

Rewards such as prestige and salary are linked in various ways with the status of an individual in the scientific hierarchy. Therefore this section begins by considering the social organization of scientists. How is the scientific hierarchy established and maintained? How does a scientist rise in the hierarchy? What factors contribute to the recognition of a good scientist? Are rewards distributed equitably according to merit? Analysis suggests that high status within the scientific community is itself an important reward, but in addition, high status brings power, useful contacts with others of high status, and additional rewards--the process of "accumulative advantage." These topics are discussed in the article by Cole and Cole.

Position within the scientific community is usually closely correlated with, and perhaps can be measured by, employment and salary. Therefore we consider next the status of women in scientific society, as indicated by recent data on employment and salary. The data presented raise interesting but difficult questions about differences in the distribution of rewards to women and men scientists.

The third kind of reward, personal satisfaction with one's work, may be less closely tied to the rewards proffered by the scientific community, because satisfaction depends more on the individual's own values. However, this reward may provide a particularly strong motivation for doing science. To illustrate the reward of personal satisfaction, we include individual biographies at the end of this subsection and elsewhere in this book. (A bibliography of additional biographies can be found at the end of this section.) The biographies also illustrate the attitudes of individuals toward their own scientific work and their varied reactions to rewards.

SOCIAL STRATIFICATION, RECOGNITION, AND SUCCESS

by Jonathan R. Cole and Stephen Cole

Excerpted from Social Stratification in Science, Chicago: University of Chicago Press, 1973, pp. 40-46, 62, 71-75.

The Contours of Stratification in Science

Most often we think of the stratification system of science in terms of a hierarchy of individuals. At the apex of this hierarchy we find the handful of men who are virtually assured important positions in the history of science. Within the recent past we think of originators of new paradigms like Einstein and Planck, or great geniuses like Bohr, Fermi, and Pauling. These are the men who shape science and give direction to the work of their colleagues. Right below this handful of geniuses are the brilliant scientists who have been awarded the most coveted of scientific honors, such as the Nobel prize or election to the various national academies of science. These men* and the relatively few others who have received widespread renown constitute the elite stratum of scientists. They total probably no more than a few thousand out of more than one million active scientists in the world. Of course, with these positions of eminence come power, authority, and control of facilities and resources. These are visible men: the tip of the iceberg. But even many Nobel laureates and National Academy members who are certainly members of the highest stratum of contemporary science may, when viewed in historical perspective, be seen as the bricklayers rather than the architects of science. The most eminent scientists reach their lofty positions through their contribution to scientific knowledge, the most highly valued form of activity in science. (1)

Another group that forms part of the scientific elite is scientific administrators. Though not having the prestige of men who entered the elite by virtue of their brilliant discoveries, administrators occupy influential positions in science. Acumen in administering science has frequently led capable but not extraordinary researchers to the highest levels of science policy-making. Scientists such as Ernest Orlando Lawrence, for one, achieved their reputations as much for their coordinating abilities as for their research skills. (2) From presidential science advisors to leaders of large research laboratories, like Brookhaven, we find powerful scientists who have reached the highest strata of the institution of science through their organizational skills. These are the entrepreneurial scientists: the men who can get large-scale projects off the ground and see them through to completion. Administrative elites also serve as "gatekeepers" to many of the government-controlled resources. (3) They determine the criteria for awarding research grants, by selecting those who meet the criteria, and they are, consequently, significant forces in determining the foci of

* The authors err throughout this article by equating scientists with men (Eds.).

scientific attention. Much of the politics of science is handled by key administrators.

We are examining two social hierarchies in which different types of functional roles lead to positions of high prestige. On the one hand we have men who achieve eminence as a result of their contributions to idea systems; on the other hand we have men who, by virtue of occupying positions of responsibility, make contributions to the organizational structure of science. (4) In the history of science, the latter type of contributors are quickly forgotten, while the former live on for generations or centuries. This selective survival may well be a consequence of the way the history of science is written. It may turn out, however, that the key "administrators" of science are as important to the advance of idea systems as are the men who make the well-known breakthroughs. Science tends, of course, to hand out significantly more recognition for contributions to the idea system of science than for contributions to its organizational development. Eponymous recognition is a case in point. Has the "father" of any scientific specialty ever been a scientific "entrepreneur"? The cases that we do identify will undoubtedly reflect the overlap between contributions to the idea and organizational systems of science. . . .

Beneath this elite are located a number of strata of lesser-known, less influential scientists. Perhaps at the top of the non-elites are those scientists who hold academic positions at high-ranked departments within their field. In American physics there are roughly 2,500 members of departments in which Ph.D.s are granted. Of these 2,500, perhaps, at the outer limit, 250 belong to the elite; the other 2,250 make up the strata right below it. Of course, there are distinctions made among these 2,250 based upon the prestige of the department to which they belong and the significance of their discoveries.

Thus far, we have considered only the small group of productive researchers, most of whom hold positions in academic settings. The mass of scientists, even those who spend some time on research, are remarkably unproductive. They produce few new ideas and rarely put anything in print. Although these scientists remain relatively invisible to the larger scientific community, they may well have achieved local prestige as excellent teachers of science or as the ranking members of their college or university science department.

There are parallel nonacademic hierarchies in science. Among government and industrial organizations, there is a loosely defined stratification along prestige lines. Scientists working for companies which give a significant proportion of their resources to basic research have greater prestige than those working for companies predominantly concerned with commercial technical applications. Companies such as Bell Telephone provide facilities and financial rewards as well as a significant quantum of recognition for research done in their laboratories. So among scientists working in industry there is a vertical hierarchy based on the "quality" of the organization that one works for. There is also, of course, a stratification system within each of these organizations. (5) Similarly, government agencies also have differential prestige. Working for the National Institutes

of Health or the Atomic Energy Commission confers greater prestige and rewards than working for less distinguished organizations such as the Naval Radiological Defense Laboratory or White Sands Proving Grounds. (6)

Continuing this broad mapping of the contours of stratification in science, we can identify in an intuitive way distinctions in the prestige of scientific disciplines and scientific specialties. . . . Physicists have recently been complaining about the "flight from physics" and the difficulty of recruiting talented youngsters. Biology appears, on the other hand, to have gained enormously in prestige among scientists and aspiring scientists since the breakthrough by Watson and Crick in the mid-fifties. The relative distribution of national resources for the scientific disciplines also seems to follow definite trends.

Within a single discipline there also appear to be variations in the relative prestige of scientific specialties. Theoretical physics tends to carry more prestige than experimental physics; high-energy particle physics seems to be more prestigious than solid-state physics. Definitions of what constitutes a prestigious specialty appear to be a product of the assessment by the field as to the areas in which significant problems exist as well as the extent to which solutions of the problem are difficult, that is, require relatively scarce talent to solve. (7)

Finally, even nations are stratified in terms of scientific productivity. Price has gone so far as to say, only half-facetiously, that nations must publish or perish. (8) While smaller nations are beginning to produce a relatively larger proportion of the total number of the world's scientific discoveries, the international scientific community (in all fields) is clearly divided into the "haves" and the "havenots." Among nations, the number of significant producers of discoveries can probably be counted on two hands. If we examine the distribution of Nobel prizes by nation, as Zuckerman has done, we see that over the past twenty years four nations, the United States, England, France, and Germany have won 74 percent of all Nobel prizes in science. (9) Evidently, whether we break down science into strata of individuals, consider it in terms of disciplines, or production of scientific ideas by nations, we see sharp inequalities in the distribution of prestige and rewards.

Throughout this description of the stratification system of science we have made frequent use of the term "prestige." This may seem indicative of the sociologist's "abnormal" concern with such unimportant matters. We must make it quite explicit that it is the value system of science which determines the prestige accruing to various scientific positions. Of all scientific activities, original contribution to knowledge is the most highly valued. With few exceptions, the prestige attached to a man or a position is a function of the extent to which they have or are expected to make original contributions. Contribution to scientific knowledge is the underpinning of the stratification system. Concern with "prestige" is not empirically separable from concern with the people contributing to scientific advance.

Thus far, we have been describing the varying strata in the stratification system of science in terms of the prestige accorded the occupants. In American society, in general, rank is based upon a variety of factors, including the possession of money, property, and prestige, derived primarily from occupation, education, or inheritance. In science, unlike society at large, location in the stratification system is not determined by the amount of money earned. Although eminent scientists generally earn more than noneminent ones, income inequality in science is probably considerably less than in business. Furthermore, the income inequality that does exist is less significant than in other institutions because in science money does not have important symbolic value. In fact, the value system of science proscribes work that is motivated by the desire to earn money.

Just as income is not a significant differentiator of scientists, neither is property. Scientific equipment and facilities are hardly ever owned by individuals. The products of science, discoveries, are also usually not owned by individuals. Very few scientists patent their discoveries. The value system of science stresses the communal nature of scientific knowledge. Although there is no property in science as we commonly use the term, scientists do possess a surrogate form of property--recognition. Recognition in science is the functional equivalent to property; and the right to "recognition" is indeed an inalienable one for scientists. (10) Recognition by peers is probably the chief motivating force in modern science. (11) Because recognition is so important to scientists, there must be a reward system that identifies and honors scientific excellence wherever it is found. If the scientist desires to acquire "property," he can do so only through recognition by the system, since there are no other legitimate ways to obtain property in science. Furthermore, illegitimate avenues to recognition are scarce. (12) Obviously, some scientists have a lot of property, or have been richly rewarded by the social system, while others remain propertyless. The scientist who is located at Harvard or Brookhaven, who is the head of the National Science Foundation, who has won a Nobel prize, or whose lifework has made a wide impact on his field, is a "rich" man in science. He has property. Correlatively, the scientist who has not been widely honored, who is located at a small, relatively unknown college, has little property.

Forms of Scientific Recognition

Recognition in science is granted in several important forms. As Merton pointed out some time ago, the institution of science has developed a reward system designed to give "recognition and esteem to those [scientists] who have best fulfilled their roles, to those who have made genuinely original contributions to the common stock of knowledge." (13) The graded forms of such recognition in science are many. . . . [They include honorific awards, appointment to prestigious jobs, and a good, widespread reputation among the scientific community.]

Are differential rewards necessary to motivate talented scientists to seek out positions requiring research? Just as this question is difficult to answer for society at large, it is difficult to answer for scientists. We

must keep in mind that the rewards scientists receive are more often recognition than material rewards. If material rewards in science were more evenly distributed, there probably would not be a sharp decline in motivation. However, failure of the system to provide recognition for work well done would probably have more serious negative consequences. Some scientists, no doubt, would continue to work hard on their research even if the norms prescribed that the researcher must remain anonymous. These scientists have the "sacred spark." They are motivated by an inner drive to do science and by a sheer love of the work. Others could probably not generate the self-discipline required to do science without the expectation of personal recognition. Most scientists are motivated, of course, by some combination of love for the work and desire for personal recognition.

If there were no differentials in distribution of recognition, research resources would be more evenly distributed. Some scientists would surely see this as a positive good. But from the point of view of the system, it is probably advantageous to have the bulk of research resources go to the men who have the greatest scientific ability. The reward system in science serves to single out those who have been successful in the past and to give them the resources to make additional discoveries. . . .

Factors that Contribute to Success

Assuming native ability among young scientists, a critical variable influencing success may be sheer work capacity. To be a successful scientist one must expend enormous amounts of energy, working long hours. Probably more important than physical stamina, however, is psychological motivation. Unlike in most other occupations, there are very few constraints on the professional behavior of scientists. (14) A university professor, for example, spends only a small proportion of his work week on formal instructional or administrative obligations. The rest of the time he is free to do what he wants. To spend a high proportion of this "free" time in self-directed research requires a good deal of self-discipline and motivation. The discipline demanded of the scientist is a strict one, requiring considerably more than putting in many hours. Thinking creatively is perhaps one of the most difficult types of work. The effort involved in pushing one's mind beyond its current limits is great. Because this work is difficult, scientists will find an unlimited number of ways to avoid it even while they are supposedly working. To be successful, a scientist must have the self-discipline to work long hours and to work productively. Such self-discipline and motivation probably explain at least as much variance on scientific success as native ability. . . .

Once we move beyond the influence of native ability, motivation, and personality factors as determinants of success, we may examine the various aspects of the social structure of science for clues to the processes by which individuals find their level in the stratification system.

Accumulative Advantage

In American science, a relatively small proportion of graduate schools produce a major portion of science Ph.D.'s; even fewer produce the

scientists who will go on to receive recognition. During the post-World War II period, from 1950 to 1966, "4 to 5 percent of the [doctorate-granting] institutions supplied 25 percent of the doctorates during the entire period; 11 percent of the institutions supplied 50 percent of the doctorates; and 24 to 26 percent supplied 75 percent of the doctorates." (15) These graduate departments are the most prestigious ones in the United States. Harriet Zuckerman has found that: "About half of all the Nobel laureates who studied in the United States received their degrees from just four universities, Harvard, Columbia, Berkeley and Princeton, which during the same period produced just 14 percent of all science doctorates." (16) Zuckerman notes that the same pattern holds for National Academy members. In 1969, 73 percent of the 710 members of the National Academy who earned their Ph.D.'s in the United States received their degrees from just ten universities. (17) What these data point to is the relationship between early location in the stratification system and ultimate success. (18)

Through processes of self-selection and social selection, most young scientists with potential attend the better graduate schools. Thus in one sense most scientists begin at the top. At an early age, they have access to some of the greatest minds in American science. The most talented graduate students have the opportunity to become working members of collaborative research teams early in their careers. Students at the top departments, judged to have outstanding potential, are kept on as faculty members, or they receive positions as junior faculty in other departments of the first rank. Those students who do not perform brilliantly (the overwhelming majority of any cadre of students, of course) are "assigned" to academic positions removed from the cutting edge of science. (There are also the few brilliant scientists who, for a variety of reasons, choose to move to less prestigious departments.) From this majority come most of the scientists who turn away from academic research; those who take jobs in industry, government, and teaching-oriented colleges. The young scientists who have been retained at top-ranked departments compete for postdoctoral fellowships, tenure positions, collaboration with world-famous scientists, and other scarce resources. Since most scientists receive their Ph.D.'s at the top graduate departments and because only the most successful receive tenure at these departments, the flow of talent in science is from the research centers to the outer fringes. It has been said that in the business world, people "rise to their level of incompetence." In science, people sink to their level of competence. (19)

Social and self-selection play a further role in the stratification process. Able young scientists seek out older eminent men to work with. Many of these young men in a hurry actually travel all over the world, often with little financial support, in their search for men with whom they can work and learn. An instructive example is provided by the experiences of James D. Watson, as he describes them in *The Double Helix*. (20) Not only did Watson seek and find Delbrück, but he traveled all over Europe in search of scientists who could help him find clues to the mystery of DNA. His attachment to the Cavendish in England could not be said to be wholly unpragmatic. In the process, Watson had a growing subterranean reputation. On the basis of his work prior to the publication

of that now famous paper in Nature, he was only a bright, aggressive young molecular biologist; yet many of the world's leading biologists were familiar with his name and knew vaguely what he was working on. Even before his pathfinding work, Watson could be said to be traveling in elite circles; his reputation truly preceded him to the ears of men like Pauling. It is, then, the informal structure of social relationships which gives the young scientist with potential, but little proven talents, the opportunity to work within the elite stratum of science.

This informal community of age-graded elites is an important aspect of the stratification system. Evidence suggests that men with talent who interact with established eminent scientists have a high probability of "making it." More than half of the Nobel laureates over the past ten to fifteen years have been students of earlier prize winners. (21) Moving from junior collaborator to senior author on important discoveries is not an uncommon process in contemporary science. We know little about how the careers of young scientists are affected by a lack of opportunity to collaborate with older, eminent scientists. It is difficult to ferret out the independent effect of individual capabilities and the effect of unequal research opportunities. A basic question remains: to what extent does a young scientist who initially does not have access to these opportunities find himself forever scientifically behind? How does the talent of this scientist get recognized? Does the system cut itself off from the young men who are the big fishes in the small ponds? Some recent data on mobility in science suggest that the process is not clear-cut. There are scientists moving into the elite departments from less distinguished ones; and an initial position in a top department does not ensure a lifetime sinecure for those scientists who fail to produce. (22)

What we really are considering here are various aspects of the process of accumulative advantage. By virtue of being in top graduate departments and interacting with influential and brilliant scientists, some scientists have a social advantage in the process of stratification. Once position has been established in this initial phase, the probabilities may no longer be the same for two scientists of equal abilities. The one who is strategically located in the stratification system may have a series of accumulating advantages over the one who is not a member of the elite corps. Indeed, these advantages may ultimately influence his position in the stratification system. This is not to say, of course, that he does not also face a series of tough tests along the way. . . .

There is another widely acknowledged advantage to being a member of a top research department or laboratory. Scientific resources and facilities are not equally assigned to research departments by those in charge of their distribution. On the contrary, there is a skewed allocation of these facilities; for the most part, prestigious research centers get a vastly disproportionate share of the wealth. The consequence for the individual scientist in these centers is a clear advantage in the production of new ideas and new publications. Of course, increased publications place these scientists in a better position for receiving future funds. The generic problem behind the concept of accumulative advantage is whether this process is an outcome of the unequal distribution of talent, which tends to

cluster at the prestigious centers, or whether "talent" is a result of the unequal distribution of resources and facilities. In either event, the process of accumulative advantage contributes to the sharpening of stratification lines.

Footnotes

1. This section on the dual hierarchies in science has gained much from informal discussions with Professor Bernard Barber.

2. Nuel Pharr Davis, Lawrence and Oppenheimer (New York: Simon and Schuster, 1968).

3. For a discussion of the role of "gatekeepers" in science, see Diana Crane, "The Gatekeepers of Science: Some Factors Affecting the Selection of Articles for Scientific Journals," The American Sociologist 2 (November 1967): 195-201; Harriet A. Zuckerman and Robert K. Merton, "Patterns of Evaluation in Science: Institutionalization, Structure and Functions of the Referee System." Minerva (January 1971): 66-100.

4. Robert K. Merton and Harriet A. Zuckerman have recently been formulating ideas on the process of "institutionalization" of scientific disciplines and have begun to emphasize the important functions performed by the contributors to development of organizational structures. For a first piece of this work, see Zuckerman and Merton, "Patterns of Evaluation."

5. Kornhauser, William, Scientists in Industry: Conflict and Accommodation (Berkeley: University of California Press, 1962); Marcson, Simon, The Scientist in Industry (New York: Harper, 1960).

6. Glaser discusses in detail the stratification of positions in a large government research organization, describing the various channels and criteria for social mobility. Barney Glaser, Organizational Scientists: Their Professional Careers (Indianapolis: Bobbs-Merrill, 1964).

7. There is an on-going debate among sociologists as to what, in fact, are the determinants of the prestige of social roles. For a discussion of this debate see pp. 60-65 and 247-53.

8. Derek J. de Solla Price, "Nations Can Publish or Perish," International Science and Technology 70 (October 1967): 84-90.

9. Zuckerman, Harriet A., Scientific Elites: Studies of Nobel Laureates in the United States (Chicago: The University of Chicago Press, forthcoming). Zuckerman has provided us with the following data. Since 1901, 268 laureates from 25 countries have been named. The United States has produced 81, Great Britain 48, Germany 45, and France 20, for a total of 194. No other country had had as many as 10 laureates (the Soviet Union has produced nine). After World War II, the United States has dominated the prize, receiving 67 of the 81 rewards after 1943. By contrast, only 10 of Germany's 45 laureates were named after World War II.

The nationality criterion used by Zuckerman is country of citizenship at the time of the prize.

10. Here we are quick to note, of course, that not all forms of property in general society are "inalienable." But, in science, recognition is truly such a possession, since without it as a rare and sought-after resource, the social structure of science would probably collapse.

11. A growing amount of qualitative information tends to confirm this point. In interviews conducted with American Nobelists, Harriet A. Zuckerman has recurring testimony that peer recognition is a major motivating force behind the work of scientists of the first rank (Zuckerman, Scientific Elites).

12. Clearly, science is not analogous here to the economy. The vast literature on deviant behavior points to alternative means to "success" within the general economic social structure. See, among others, Merton, "Social Structure and Anomie," in Social Theory and Social Structure (Glencoe, Illinois: The Free Press, 1957); Richard Cloward and Lloyd Ohlin, Delinquency and Opportunity (Glencoe, Illinois: The Free Press, 1960).

13. Merton, "Priorities in Scientific Discovery: A Chapter in the Sociology of Science," American Sociological Review 22 (December 1957), especially pp. 639-47. The significance of recognition in the social institution of science has been further examined by F. Reif, "The Competitive World of the Pure Scientist," Science 134 (December 1961): 1957-62; Glaser, Organizational Scientists; Hagstrom, The Scientific Community; Crane, "Scientists at Universities."

14. This discussion applies only to academic scientists.

15. Publication 1489, Doctorate Recipients from United States Universities: 1958-1966 (National Academy of Sciences, Washington, D.C., 1967).

16. Harriet A. Zuckerman, "Stratification in American Science," Sociological Inquiry 40 (Spring 1970): 235-57.

17. Don E. Kash, et al., "University Affiliation and Recognition: National Academy of Sciences," Science 175 (10 March 1972): 1076-84.

18. For further treatment of the influence of graduate education and social location on the destiny of the scientist in the stratification system, see Diana Crane, "The Academic Marketplace Revisited," American Journal of Sociology 75 (May 1970): 953-64; Lowell Hargens and Warren Hagstrom, "Sponsored and Contest Mobility of American Academic Scientists," Sociology of Education 40 (Winter 1967): 24-38; Lowell Hargens, "Patterns of Mobility of New Ph.D.'s among American Academic Institutions," Sociology of Education 42 (Winter 1969): 18-37; Bernard Berelson, Graduate Education in the United States (New York: McGraw-Hill, 1960).

19. William J. Goode, "The Protection of the Inept," American Sociological Review 32 (February 1967): 5-18.
20. James D. Watson, The Double Helix (New York: Atheneum, 1968).
21. Harriet A. Zuckerman, "The Sociology of the Nobel Prizes," Scientific American, 217 (November 1967): 25-33.
22. Hargens, Lowell, and Hagstrom, Warren, "Sponsored and Contest Mobility of American Academic Scientists," Sociology of Education 40 (Winter 1967).

Recognition

The forms of recognition given to scientists include (1) prizes, the best known of which is the Nobel prize; (2) election to honorary societies with restricted membership, such as the National Academy of Sciences or the Royal Society of London; (3) invitations to present a discussion of their work at national or international meetings; (4) invitations to write review articles on a speciality; (5) election to office in a national or international society in their field; (6) publication of their work in prestigious journals; (7) citation of their work by others, both scientists and reporters; and (8) promotion to positions of greater prestige, in industry or in academe.

Theoretically and idealistically, recognition is based on the scientific merit of a person's work. However, it may be difficult to disentangle judgments of merit, which are subjective value judgments, from the influences of personal friendships, the aura derived from association with prestigious institutions, and the influence exerted by the individual in proclaiming the merits of his or her results or clever idea. In practice, women may encounter more difficulty than men in obtaining recognition for their accomplishments, although the difficulties are now not as great as in the past. For example, the Royal Society of London, founded in 1660, first elected women to membership in 1965 (Lonsdale, 1970). The National Academy of Sciences of the United States, founded in 1863, admitted its first woman, Florence Sabin, in 1925. In 1977, 28 out of 1,219 members of the National Academy were women. Seven women have won one of the most famous awards, a Nobel prize (Table 1; cf. article on DNA; Zuckerman, 1977). It should be pointed out that these honors are mentioned because they are well known. Equally meritorious workers, both men and women, have not been equally honored.

Table 1. Female Winners of the Nobel Prize*

<u>Name</u>	<u>Country</u>	<u>Awarded</u>	<u>Field</u>
Marie Curie	France	1903	Physics
Marie Curie	France	1911	Chemistry
Irene Joliot-Curie	France	1935	Chemistry
Gerty T. Cori	U.S.A.	1947	Physiology-Medicine
Maria Goeppert-Mayer	U.S.A.	1963	Physics
Dorothy C. Hodgkin	England	1964	Chemistry
Rosalyn S. Yalow	U.S.A.	1977	Physiology-Medicine

* Nobel prizes were first awarded in 1901; in 1969 a new prize in Economics was added. Including 1977, awards have been given to 109 people in physics, 89 in chemistry, 121 in physiology-medicine, and 14 in economics.

As an example of the process by which recognition is accorded, consider the National Academy of Sciences. New members are elected annually by the present members. The following account comments on the process and its outcome.

NATIONAL ACADEMY OF SCIENCES: HOW THE ELITE CHOOSE THEIR PEERS

by Phillip M. Boffey

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Philip Handler, president of the National Academy of Sciences, was drawing to the close of his annual report to the members on 26 April when he touched on the subject of election to the prestigious academy. Just that morning the members had completed their secret, mysterious, year-long election rites by voting in 60 new members "in recognition of their distinguished and continuing achievements in original research." Now Handler took note that there are pressures on the academy to broaden the pool of scientists from which it selects members. "The external world views us with a cocked eyebrow," he warned. "Egalitarianism and populism are in flood. There are those who would urge upon us public nomination of candidates for membership in the Academy. . . . There are . . . those who would have the membership of the Academy reflect the proportions of various groups within the population; by states, by sex, by ethnic groups. And there are those who consider the Academy to be an elitist relic of the past.

"Perhaps so," Handler continued. "But the hallmark of the Academy must continue to be excellence in all things and we must, above all else, retain our single criterion for election. We do believe that in science the best is vastly more important than the next best." A few sentences later Handler concluded. The members rose to give him a sustained standing ovation--the most heartfelt applause, some say, that Handler had ever received for an annual report. His defense of excellence and of the academy's ability to recognize it had touched a responsive chord.

But how well does the academy's labyrinthine elections process actually perform when it comes to selecting the most accomplished scientists in the land for membership? "That's a valid question," says David R. Goddard, the academy's home secretary and chief elections official. "I wish to hell I could give you an objective answer to it. We don't have an objective measure that I really know of." Goddard said it is his subjective opinion that "you can always find people outside the Academy who are just as good as the Academy members." But he described the number of "mediocre people" who gain admission to the academy as "minimal--there are relatively few Academy members that one would say shouldn't have been elected."

However, with the academy membership now exceeding 1200, he added, even a 10 percent error rate "would leave you with a large number of ordinary people."

One imperfect measure of the academy's perspicacity is the extent to which it recognizes and honors talent of Nobel prize quality. (The measure is imperfect, among other reasons, because the Nobel judges are by no means infallible.) According to Goddard, "the Academy often has a red face" when the Nobel awards are announced and it turns out that some American scientist has been honored who is not yet a member of the academy. . . .

Many of these individuals appear to have been missed because they came from disciplines or institutions that were not strongly represented in the academy. Thus, in the past few years, the Nobel prize has been awarded to such non-academicians as Simon Kuznets and Wassily Leontief, both economists, a field in which the academy has only recently been building up its strength; Leon N. Cooper, a physicist from Brown University, outside the mainstream of elite institutions which dominate the academy; and Ivar Giaever, an applied physicist from industry (General Electric), who was thus outside the community of pure academic scientists who predominate in the academy. In all of these cases, the academy subsequently elected the Nobelist to membership. Another industrial scientist who may have been overlooked unjustly for many years is Lewis H. Sarett, a chemist and president of Merck Sharp & Dohme Research Laboratories, who synthesized cortisone by various routes in 1944 and 1952 and won the National Medal of Science last year; he was finally elected to the academy this year. On an overall basis, however, Goddard believes that the academy does a respectable job in selecting the nation's most outstanding scientists. He notes that some 68 living academicians have won Nobel prizes and that "relatively few" of these got their Nobel award before their academy membership.

The chief criterion for election to the academy has traditionally been outstanding original research work. But there are no written guidelines defining just what a scientist must do to qualify, and other factors sometimes influence the academy electorate.

Some individuals appear to be elected because of their eminence as administrators of major institutions on the scientific scene. This year, for example, the academy elected Harold Brown, the Carter Administration's Secretary of Defense. Goddard says that the process that led to Brown's election was well under way before it was realized that Brown would become Secretary of Defense. Even so, Brown had been out of active research for many years and had largely made his reputation in a series of important administrative posts, including that of director of defense research and engineering for the Pentagon, Secretary of the Air Force, and president of Caltech. He joins a number of other academicians who are probably better known for their administrative accomplishments than for their research contributions, including H. Guyford Stever, former director of the National Science Foundation; James A. Shannon, former director of the National Institutes of Health; and S. Dillon Ripley II, secretary of the Smithsonian Institution.

The membership of the academy consists primarily of post-middle aged white males from a relatively small number of elite institutions. As of last year, some 60 percent of the members were 60 years or older and another 25.6 percent were 51 to 59. The number of blacks elected has been miniscule (no records are kept of a member's race). And there are currently only some 28 women in a membership of 1219.

Academy officials say the lack of women reflects the lesser role of women in the scientific community at large, but feminists see evidence of male chauvinism at the academy. Years ago the academy was embarrassed by an obvious injustice to the female half of a distinguished research team. In 1940, Carl F. Cori was elected to the academy. In 1947, he and his wife, the late Gerty T. Cori, shared a Nobel prize in physiology or medicine with an Argentine scientist. In 1948, the academy belatedly woke up and elected Gerty to membership. Similarly, partisans of anthropologist Margaret Mead were annoyed for years that she had not made the academy, but were mollified when she finally won election in 1975. In the most recent election, 4 of the 60 new members were women.

Geographically, according to Goddard, the academy members are concentrated in three areas--the eastern seaboard between Washington, D.C., and Cambridge, Massachusetts; the state of California; and a midwestern region that includes Illinois, Wisconsin, Michigan, and areas immediately adjacent. As of 1976, 12 states had no members at all--Alaska, Arkansas, Hawaii, Idaho, Kentucky, Louisiana, Mississippi, Montana, North Dakota, Oklahoma, South Dakota, and Wyoming. The academy says its membership reflects the distribution of scientific talent in the country, but scientists from some of the have-not states find it hard to believe there is not a single scientist in their areas who is as good as some of the academy members.

There appear to be no up-to-date analyses of the institutional affiliations of academy members. A computer printout that tabulates the number of academicians who were employed full time at various universities in October 1976 reveals that Harvard had the greatest number, 98, followed by Berkeley, 67; MIT, 64; Stanford, 51; University of Chicago, 45; Caltech, 44; Rockefeller University, 39; University of California at San Diego, 34; University of Wisconsin, 31; and Yale, 27. Just missing the top ten were Cornell and Illinois, with 24 apiece. This year the rich got richer. Harvard claimed the most new members (6), followed by Stanford (5), and MIT (4). Also this year Bell Laboratories, the National Institutes of Health, and the University of California at Los Angeles had three apiece.

By most accounts, election to the academy is second only to the Nobel in the esteem accorded to it by most of the American scientific community. Universities boast of the number of academicians on their faculties, and the individual who wins membership in the academy is said to have an enhanced bargaining position when it comes time to bargain for a new job or a higher salary. Thus the elaborate, closely guarded elections process excites considerable interest and speculation among the upwardly mobile segment of the scientific community that aspires to academy membership. "I'll probably be criticized by some members for even talking to you," Goddard told Science as he launched into a description of how the process works.

As Goddard sees it, there are two main elements--the process by which an individual is nominated, and the process of election. The chief role in nominations is played by the academy's 23 disciplinary sections--covering such areas as mathematics, physics, genetics, and economic sciences--to which academy members are assigned at their own choice. The existing members of a section generate the names of new candidates for membership, review a list of each candidate's most important scientific articles and a 250-word summary of his major accomplishments, and conduct a series of informal and formal ballots. At the end of the process, all persons who have received the votes of two-thirds of the members voting in any one section are considered nominated. The vast majority of nominations--probably more than 80 percent in a typical year, according to Goddard--emerge in this way.

The process has been criticized for being incestuous because many of the sections tend to be dominated by faculty members from a handful of university departments--a circumstance that causes scientists at other institutions to grumble occasionally about institutional favoritism in the nominations process. But the academy has also established other avenues for nomination. Any five members of the academy can propose that a prospect be treated as an intersectional candidate, in which case the prospect needs only 50 percent of the combined vote in two sections to be nominated instead of two-thirds of the vote in one section. A group of 20 or more members of the academy can set themselves up as a "voluntary nominating group" and propose their own candidate, who is thereupon deemed an official nominee. (No more than five members of the group can be affiliated with the same institution, a provision designed to prevent the institutions that dominate the academy from steamrolling their colleagues in.) And the academy's governing council occasionally forms "temporary nominating groups" to develop lists of candidates in areas where the academy is known to be weak. This year, for example, a temporary group for the area of history and philosophy of science sparked the election of Martin J. Klein, historian of physics at Yale; Otto E. Neugebauer, historian of mathematics at Brown; and Willard van Orman Quine, philosopher at Harvard. Another such temporary group has been operating in geography for four years. The academy council itself has the power to make nominations, but has not exercised that privilege for more than a decade.

The academy makes no effort to publicize it, but there is even a mechanism whereby the general public can feed names into the hopper. Goddard says there is nothing in the bylaws to prevent the submission of names by outsiders; in fact, the home secretary occasionally gets a letter from some outsider nominating himself or someone else. If it is "not obviously a screwball nomination" and involves someone working in areas relevant to the academy, Goddard says he will forward the name to one or more sections for consideration as a possible nominee. Goddard, who has been in the academy for 26 years and has served as home secretary for the past two elections, says he "can't recall a case where a person has been elected from an outside nomination but I believe there have been such cases--we'd have to search the records."

Once all the nominations are in, the most crucial elements in the elections process--the five class membership committees--swing into operation. These committees, composed of a dozen or more academicians, are responsible for broad areas of science, each of which encompasses several of the disciplinary sections. (The biological sciences class committee, for example, presides over biochemistry, botany, and five other disciplinary sections.) Each class committee prepares a rank list of the nominees that have emerged from the sections under its jurisdiction. For the most part they just interdigitate the nominees from various sections, deciding whether the nominee with the most votes from the chemistry section should rank ahead of the leading physics nominee, and so forth. But they also impose their own judgment on the process and occasionally differ markedly from the sections in evaluating a particular individual. A vigorous partisan or opponent on the class membership committee can mean the difference between an individual's eventual election or defeat. The committees are assigned quotas that they may not exceed, and thus in effect eliminate many of the names from further consideration and forward the rest, in serial order of the committee's preference, to be voted on by the membership. In the most recent election, some 150 persons were nominated, of whom 90 (the quota limit) survived scrutiny by the class committees. Under bylaws previously adopted, only 60 of those 90 could be elected to membership.

The major ballot is then conducted by mail. Each member of the academy gets a packet of material that includes the rank list of each class membership committee, a summary of each candidate's accomplishments, and a record of the voting by the nominating groups that initiated the whole process. Each member is supposed to vote for from one-third to one-half of the names on each of the five class rank lists. Since many academicians know few of the candidates outside of their own discipline, they tend to follow the suggested rank lists or else, some say, they vote on the basis of institutional loyalty, assuming, for example, that the chemist from their own university must be better than the chemist from another school. One academician told Science he finds the process so complicated and the idea of just blindly following the committee rank lists so distasteful that he doesn't bother to vote.

The end product of the mail ballot is yet another rank--the nominees are listed in order of votes received, with suitable adjustments made to ensure that each class receives its proper quota. The final ballot then takes place at the annual meeting, at which time there are various mechanisms whereby those members present can make a last-ditch try to boost or block a particular candidate. Six years ago, the council of the academy successfully blocked the election of Lamont C. Cole, a Cornell ecologist, causing a furor that reached public attention. Since then, the elections have been relatively quiet. This year the only questions raised from the floor concerned the qualifications of William R. Hewlett, president and chief executive officer of the Hewlett-Packard Company. But after it was explained that Hewlett was being nominated for his contributions to scientific instruments that have revolutionized some fields of science rather than for basic research contributions, the questioner seemed satisfied, and Hewlett won final election--along with the other top 59 on the list--with no difficulty.

There have occasionally been suggestions that the academy should cast its electoral net wider, either by encouraging nominations from outside the academy or even--to the horror of most academicians--allowing nonmembers to vote in some fashion. But the academy, jealous of its prerogatives and its reputation for excellence, remains unapologetically elitist.

Employment and Salary

Although recognition and the "property" inherent in discovery may be more important than salary, these different forms of reward for scientific work are not unrelated. We consider next employment in science, with particular attention to the participation and salaries of women. The salary data can be used in two ways: to measure the status of women in the scientific hierarchy, and to assess the potential remuneration of a career in science. It is necessary to note, however, that individual experiences may differ from the mean, that certain specialists may be more in demand and hence receive higher salaries than others, and that the employment picture changes frequently.

WOMEN IN THE NATURAL SCIENCES

by Betty M. Vetter

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Women scientists, long underrepresented, underpromoted, and underpaid in their fields relative to men, have not achieved parity of opportunity despite the forces of affirmative action. The gains in the participation of women in the U.S. scientific enterprise are pitifully small, and the obstacles still standing in the path of those who wish to enter and participate fully in that enterprise always have been and still are enormous.

American women have never been strongly represented in the physical sciences, although they have made up a significant portion of the bioscience work force, even at the highest levels of education, and are well represented in the social and behavioral sciences. Their participation in science is increasing slowly, after a lag of several decades.

In the 1920s, women earned one of every twelve doctorates in physical science and mathematics, and one in every six doctorates in bioscience, but these proportions fell during the 1930s and continued to fall during the 1940s and 1950s, when women earned only one of every twenty-five doctorates in the physical sciences and one of every eleven in the life sciences. During the decade of the 1960s, the proportions started to rise slowly, and in the 1970s, the rise has been more rapid. (1) In 1974, women earned one in ten doctorates in chemistry and mathematics, one in twenty-four in physics, one in five in the biosciences and the social sciences. The proportions range from one in three in anthropology and psychology to one in ten in economics. (2) The proportions of women enrolled in graduate programs in these areas are even higher so that a

slowly increasing proportion of the new scientific work force in the last half the 1970s will be women. However, while the proportion of women enrolled and graduating in these fields is increasing, their opportunities for employment and advancement as scientists in industry, government, or academe continue to be limited.

A falling employment market for new Ph.D.'s has created problems for men as well as for women, but the relative position of new women doctorates has deteriorated over the past several years. Whereas 5 percent of men and 10 percent of women doctorates in 1965 were still seeking jobs at the time of the Ph.D. award, the proportion had risen to 14 percent of the men and 20 percent of the women by 1973. (3) The unemployment rates for women in science (and indeed for women in all fields of specialization) continue to be two to five times higher than for men in the same field with comparable training and experience.

While the number of women scientists who are unemployed is small in comparison to the numbers of unemployed women professionals in all fields combined, their significance should be viewed in the context of continuing, loud protests by potential employers in industry and higher education that "qualified women scientists" cannot be found.

Some improvement can be noted overall in opportunities for women. However, new studies (4) continue to show that opportunities for women scientists have not grown as fast as their increasing participation in the labor force, leaving them relatively further behind men than they were a decade ago. Women scientists not only have more difficulty finding employment than do men with comparable education and experience, they also find it more difficult to advance. Their salaries are lower than those of comparable men in every employment setting, at every degree level, and at every level of experience. Salaries for women Ph.D.'s in science and engineering who were under thirty years of age in 1973 were \$1,600 below salaries of men in the same age bracket, according to a National Research Council study published in 1974. (5) At age forty, women's salaries lagged \$4,300 behind men's, and by age fifty, Ph.D. women in science received \$6,200 per year less than men.

A similar salary differential exists for men and women at lower degree levels. For example, offers to new women baccalaureate graduates in chemistry and in mathematics in the 1974-75 recruiting year averaged \$216 per year below offers to men; in the biological sciences, \$768 less; and in other physical and earth sciences, \$1,332 less. Only in engineering did salary offers to women slightly exceed those made to men. (6)

Specific Fields of Science

In chemistry, women have earned one in five bachelor's and master's degrees and one in fourteen Ph.D.'s granted since 1960, rising to one in eleven in 1974. However, the 1973 chemistry faculty at institutions awarding the doctorate included only two women in every 100 above the level of instructor, and the federal work force of chemists at all degree levels in 1974 included less than one in seven women. (7) In 1975, the

salaries of women chemists remain consistently below those of men. (8) At the bachelor's level, women chemists earn from 73 percent of men's salaries in educational employment to 84 percent in government. Women with master's degrees earn only 76 percent of men's salaries in industry but 90 percent in government. At the doctoral level, women are paid 82 percent of men's salaries in industry and 83 percent in educational institutions.

When salaries are tabulated by type of work activity instead of by education level, the same differentials occur with women's salaries ranging from 64 percent of men's in marketing to highs of 82 percent and 83 percent in research and development. Among jobless chemists seeking employment in 1975 were 1.5 percent of the male members of the American Chemical Society and 2.7 percent of the women members. In 1974, the unemployment rates were 1.2 percent for men and 3.5 percent for women. The higher unemployment rates for women persist at all degree levels. Among chemistry doctorates in 1973, 1.8 percent of the men and 8.9 percent of the women were unemployed and seeking jobs. (9)

The situation in physics is similar, although the proportion of women in the field is smaller at all degree levels. Women have earned only one in every forty-four of the physics doctorates, one in nineteen of the master's degrees, and one in eighteen of the bachelor's degrees since the 1950s. (10) The proportions are increasing, but not rapidly. In 1974, women earned one in eleven of the bachelor's degrees, one in twelve of the master's degrees, and one in twenty-five of the Ph.D.'s. (11) One of every fourteen physics graduate students enrolled in 1973-74 was a woman, as were one in twelve of first-year graduate students. (12) Women doctorates in physics had an unemployment rate of 7.4 percent in 1973, while the rate for men was 1.7 percent. (13) No more recent data are available. In 1973 the American Institute of Physics found 5.2 percent unemployment among women Ph.D.'s and 1.1 percent among men, while the rates at the master's and bachelor's levels were 1.9 percent for men and 5.0 percent for women. (14) The salaries of women Ph.D. physicists averaged \$17,830 in 1973, compared to \$21,220 for men. (15)

In the geosciences, women have earned one in forty-one doctorates since 1960, one in twelve master's, and one in ten bachelor's degrees. (16) In 1974, the proportions had risen to one in twenty-six doctorates, one in nine master's, and one in six bachelor's. (17) Wide differentials exist throughout the geosciences between men and women, not only in degree of participation, but in salaries and type of employer. Industry employs 43 percent of the men but only 18 percent of the women. Colleges and universities employ one-fourth each of men and women, but 27 percent of women geoscientists teach in secondary schools compared with 5 percent of the men. Salary levels differ by type of employer, but women's salaries are well below those of men in all employment settings and at all degree levels. For example, in 1974, median salaries for women at the bachelor's level working for the petroleum industry were \$14,100 while comparable men made \$25,100. Women with Ph.D.'s working in colleges and universities had median salaries of \$14,000, while comparable men earned \$17,800. Among geoscientists working for the federal government in the U.S. Geodetic Survey, women with master's degrees earned median salaries of \$16,000, while men earned \$21,000. (18)

The biosciences, where women have earned about one-third of the bachelor's and master's degrees and one-fifth of the doctorates since 1970, show similar patterns, except that some correlation may exist between the fact that women are more strongly represented in the life sciences, and salaries for life scientists generally are lower than for physical scientists. The bioscience fields are the fastest growing areas of enrollment at both undergraduate and graduate levels, (19) and women are increasing their proportion more rapidly in this than in other areas of science. Almost 30 percent of all graduate students in the biosciences in 1973-74 were women. (20)

Increasing participation of women in the sciences might seem assured as schools and the general society drop some of the social barriers that have turned girls away from these male-dominated fields. However, indications from college freshmen as to their intended majors are not altogether supportive of this assumption. The proportion of freshman women who indicated their intention to major in the physical sciences dropped from 1.2 percent of the class in 1966 to 0.8 percent in 1971 and 1972, rising back to 1.2 percent in 1974. Interest in the life sciences, on the other hand, climbed from 3.2 percent of freshman women in 1966 to 7.2 percent in 1974, while the social sciences as intended major have dropped from 11.7 percent of freshman women in 1966 to 9.3 percent in 1974. (21)

Interest in the physical sciences also has dropped among male freshmen, thus leaving more room for women in those areas where women have been least represented and where job opportunities are most likely to be best. Nonetheless, women apparently are not being sufficiently encouraged to enter these fields.

Certainly there is little in the information available about those exceptional women who have chosen and pursued careers in science (or indeed in other fields) to encourage young women to follow in their footsteps. Women scientists have been less likely than men to obtain employment in prestigious educational institutions or in private industry; and once employed, they have not been advanced to managerial responsibility or higher academic rank at rates resembling men's. Even in their personal lives, those women who have aspired to careers at the doctoral level in the demanding areas of science have faced greater difficulty in combining marriage and family with a career than have their male cohorts. A recent study by John Centra found that more than a third of a sample of women doctorates from the 1960s never married, and among those who did, a distressing 25 percent were either separated or divorced in 1973, compared with only 10 percent of the comparable men. (22) We have no comparative data on women science Ph.D.'s in terms of their marital stability so that we might know whether women in science differ in this characteristic from all women Ph.D.'s.

Cultural tradition, sex-role expectations, and discriminatory practices have played and still play a large role in the training of women at all levels and in their employment opportunities. Particularly in the sciences, re-employment opportunities for women who wish to leave the work force for a time to have and raise a family are hazardous. Part-time employment in

science is not readily available under most circumstances. It is not surprising, then, that girls who have few role models in science to emulate are not choosing to receive training in those areas where potentially they are most needed. Further, the proportion of women in science is not likely to increase substantially until women are actively encouraged to consider such careers at an early enough stage that they do not shut doors by inadequate program choices in the junior high and senior high school years.

Among those women who, against considerable odds, have chosen science careers and are now in the work force with exceptionally fine credentials, some have found that barriers to progress in their professional careers are dropping. Others still struggle, not so much with the blatant sexism they faced a few short years ago, but with the subtle barriers put in their way. Women scientists who are unable to participate socially with their male colleagues are not included in some of the exciting plans or discussions that lead to a new discovery and ultimate publication. Women who are not offered the opportunity to serve on prestigious committees of scientists discussing science (but are asked, instead, to serve on committees devoted to affirmative action, the status of women, or improving the lunchroom) are unlikely to share in development of those scientific projects which will result in publication. Not only in academic institutions, but in government and industrial laboratories, publication is an inescapable rung on the ladder of advancement.

Even women who are key participants in new scientific discoveries are likely to lose credit for their achievements, as we have learned from the cases of Jocelyn Bell Burnell, whose findings formed the basis for the discovery of pulsars, (23) and of Rosalind Franklin whose basic work on X-ray diffraction patterns of the B-chain of DNA and whose measurements and interpretations of them were essential in finding the key to the structure of the DNA molecule. (24) Burnell was a graduate student at the time of her discovery, while Franklin was an experienced researcher; but the ultimate result was the same--both were robbed of the credit they so obviously deserved in a major scientific discovery. The reward for scientific achievement (from Nobel and other prestigious prizes to recognition and esteem from colleagues and the lay community) are so high that even otherwise honest men have succumbed to the temptation to steal credit from others. Stealing such credit from a woman has always been easier than stealing from a man and may in part account for the fact that the names of so few women scientists between Madame Curie and Margaret Mead can be recognized by ordinary literate people.

Another obstacle to ultimate accomplishment of the goals of equal opportunity for women in science (and other professional areas as well) is the publication by prestigious individuals or groups of assertions that such equality already has been attained. For example, a new report by the Carnegie Council on Policy Studies in Higher Education concludes that "there is now no gap between [the supply of presently qualified women] and those holding 'ladder' positions on faculties. Women constitute 16 to 17 percent of recent recipients of Ph.D.'s and hold 18 percent of the faculty ladder positions in four-year colleges and universities." (25) The report fails to stress that only 6.3 percent of professors in universities, while

23.9% of the assistant professors, are women. In four-year colleges, 12.9 percent of the professors and 28.1 percent of the assistant professors are women. However, being in a "ladder" position does not in any way guarantee that tenure will be achieved. In 1974-75, 56.9 percent of the male faculty in all institutions held tenure, compared with only 27.0 percent of the female faculty. (26) We have no reason to believe that women science faculty members are more likely to hold tenure than women in other departments--indeed there is some indication that the proportion in science may be even lower. The academic job market is not expected to expand over the next decade or so, and thus tenure will be much more difficult to achieve from a "ladder" position.

Another recent study, by Allan M. Carter and Wayne Ruhter, misleadingly titled The Disappearance of Sex Discrimination in First Job Placement of New PhDs (see n. 3), does not show that discrimination has disappeared even in first placements, except possibly for those women able to find academic employment. Yet the appearance of such studies, with such statements or inferences, provides a comforting base for those who prefer to stop trying to improve the status of women.

Summary

The status of women in science is about what it was five years ago, before "affirmative action" was mandated by executive order. There are some gains in the proportion of women among new graduates in science and even larger gains in the proportion of women enrolled in graduate programs in science. A few women have been promoted into positions of greater responsibility, both in industry and in academic institutions, but science remains a man's world, dominated by a male fellowship in which few women have an opportunity to participate fully. No women scientists can yet be elected to membership in the prestigious Cosmos Club in Washington, D.C.; only twenty-five women are among the 1,134 living members of the National Academy of Sciences, and six of these twenty-five were elected in 1975. Among these are only one chemist, two physicists, and one ecologist.

Women in science have come a few short steps in the past few years, but the goal of equal opportunity and equal reward for women lies somewhere down the distant path into the future.

Footnotes

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2. "Earned Degrees, 1973-74," unpublished preliminary data, National Center for Educational Statistics, Washington, D.C.

3. Allan M. Carter and Wayne E. Ruhter, The Disappearance of Sex Discrimination in First Job Placement of New PhDs (Los Angeles: Higher Education Research Institute, 1975), p. 18.

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5. Commission on Human Resources, National Research Council, Doctoral Scientists and Engineers in the United States--1973 Profile (Washington, D.C.: National Academy of Sciences, 1974), p. 25.
6. A Study of 1974-75 Beginning Offers, Final Report (Bethlehem, Pa.: College Placement Council, July 1975), p. 3.
7. Vetter and Babco, pp. 171, 185, 196.
8. "1975 Salary Survey," Chemical and Engineering News 53, no. 25 (June 23, 1975): 20-23.
9. Commission on Human Resources, p. 25.
10. Vetter and Babco, p. 225.
11. "Earned Degrees, 1973-74" (n. 2 above).
12. 1973-74 Graduate Student Survey, AIP Publication no. 207.7 (New York: American Institute of Physics, July 1975), p. 2.
13. Commission on Human Resources (n. 5 above), p. 25.
14. Beverly F. Porter, Sylvia F. Barisch, and Raymond W. Sears, "A First Look at the 1973 Register," Physics Today (April 1974), pp. 23-32.
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16. Vetter and Babco, p. 253.
17. "Earned Degrees, 1973-74.
18. "Professional Characteristics of Geoscientists," Scientific, Engineering, Technical Manpower Comments 12, no. 2 (March 1975): 3-4.
19. Betty M. Vetter, "Employment Opportunities for Natural Scientists and Engineers in the '70s," Science Education News, American Association for the Advancement of Science (April 1973).
20. Vetter and Babco, p. 25.
21. Ibid., p. 21.
22. Centra (n. 4 above), pp. 101-9.
23. Nicholas Wade, "Discovery of Pulsars: A Graduate Student's Story," Science 189 (August 1, 1975): 358-64.

24. Anne Sayre, Rosalind Franklin and DNA (New York: W.W. Norton & Co., 1975).

25. Carnegie Council on Policy Studies in Higher Education, Making Affirmative Action Work in Higher Education (San Francisco: Jossey-Bass, Inc., 1975), and press release dated August 11, 1975.

26. Vetter and Babco, pp. 147-49.

WHY ARE WOMEN SCIENTISTS PAID LESS THAN MEN?

by Ann E. Kammer

The custom of paying female scientists less than males performing lesser or equivalent work has a long history. According to an 1889 report, Maria Mitchell, an astronomer and the first woman scientist appointed to a college faculty,

accepted the position [at Vassar] and the salary offered her, without her thinking to inquire whether the salary was the same as that given to other professors. It was the chance to work that she wanted, the chance for influence in one of the first colleges for women. The money she was to receive was a minor consideration, and quite as characteristic was her indignation when, after being there for a considerable time, her attention was at last called to the fact that she, a mature woman, with a European fame, was receiving a salary less than that paid to some of the professors who were young men, almost entirely without experience, and quite destitute of reputation. The indignant protest, which then called for an equal salary, was not a personal affair. She flamed out in behalf of all women, and of abstract justice, with a glow which forced an immediate increase in salary. The excuse for this injustice must be found first in the fact that, at the time when Vassar College was established, women had not proved what they can do in professional lines, and second, in the very conservative influences which guided the policy of the institution. (1)

Recent studies show that the salaries of men and women continue to be unequal. Two examples are summarized in a June 1977 publication of the Project on the Status and Education of Women (2):

The National Center for Educational Statistics reported (3) recently that salary inequalities between women and men faculty members have increased in the past year. Female professors earned \$3,096 less, on the average, than males in 1975-76, compared to \$2,820 less in 1974-75. The dollar gap between average salaries for women and men is the greatest in the higher ranks. In addition, the total percentage of faculty jobs held by women increased by only one-half of 1% between 1974-75 and 1975-76 (from 23.8% to 24.3%). The center's final figures on faculty salaries for 1975-76 echo the findings of the annual salary survey of the American Association of University Professors that little overall progress toward job equality for faculty women was made in the past year.

The Labor Department's Women's Bureau publication, The Earnings Gap Between Men and Women, reports that, among year-round full-time workers, men's earnings exceeded women's by 56% in 1955. By 1974 this differential had risen to 75%. Median 1974 earnings of women were \$6,772, compared with \$11,835 for men. In addition, only 5% of workers earning \$15,000 or more were women. Two factors cited as contributing to the widening gap were the continued concentration of women in low-skilled, low-paying occupations; and the rapid increase in the number of women in the work force, with large proportions of women in or near entry-level jobs. (4) [Note that these studies deal with all professions and jobs, not just scientific occupations.]

More recent data, obtained from the 1975 Survey of Doctoral Scientists and Engineers, provide similar evidence of the differentials between the salaries of men and women bioscientists. (5)

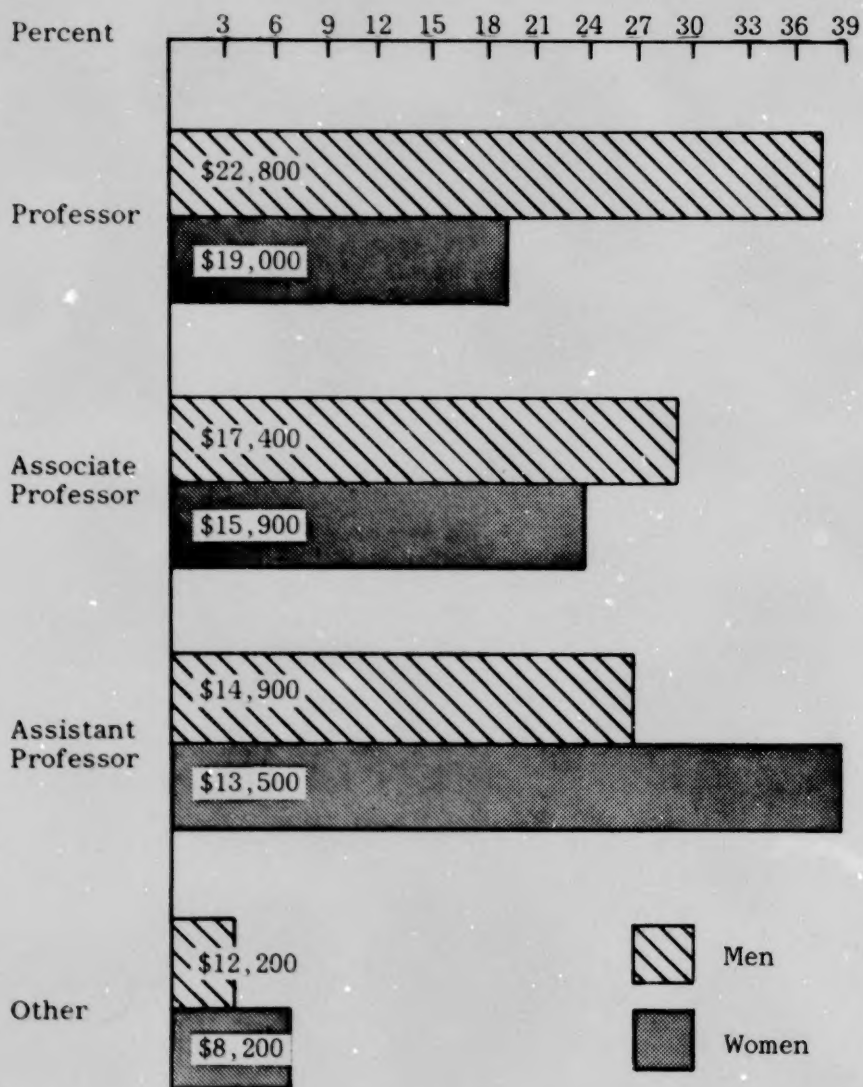
What causes the difference between the mean salaries of men and women scientists? Some answers are discussed in a 1975 government report prepared by George E. Bowden, III. (6)

One explanation is that the numbers of women scientists have increased only recently, and therefore there is a higher percentage of young, and hence lower-paid, women scientists in the work force. However, differences persist when comparisons are made between men and women who received their doctorate in the same years. In colleges and universities women are most numerous in the lower academic ranks, and even at comparable ranks receive lower salaries (see Illustration 1, which is taken from the report). The report continues:

A second possible explanation arises from the generally held supposition that few women devote their whole lives to their careers, having to take time out for child bearing and rearing. If this is the case, then one would expect that it would take longer for a women to achieve comparable pay just because she is dedicating a large chunk of time to her family rather than to her profession. However, indications are that this does not have much impact on explaining the situation. Only three-fourths of women life scientists with Ph.D.'s have ever married (against 95 percent of the men) [1970 census data]; this partially dilutes the effect those with family responsibilities have on the whole population of women bioscientists.

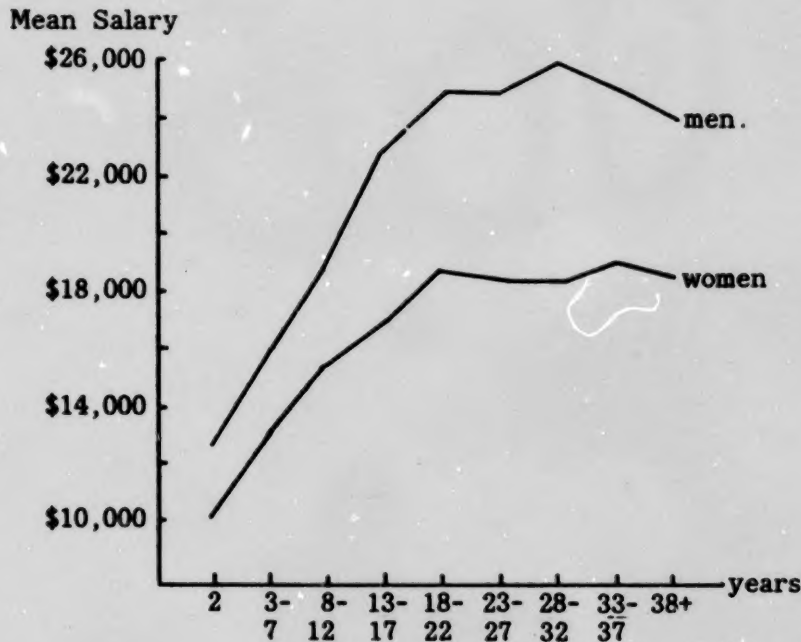
Furthermore, if this hypothesis were true, its effects would appear when mean salary is graphed against years in the doctoral bioscience manpower pool (i.e., years since receipt of doctorate). As can be seen from Illustration 2, however, the salary curves for both men and women level off at 18-22 years in the pool, and the women level off at a

Illustration 1. Percent Distribution of Male and Female Bio-scientists by Academic Rank (Dollars = Average Salary)



Source: Survey of Doctoral Scientists and Engineers (SDSE) file.

Illustration 2. 1973 Mean Salary vs. Years in Doctoral Bioscience Pool



significantly lower plateau than the men. This indicates that women are unlikely to achieve pay comparable to that received by men no matter how long they work. In other words, by the time one has been in the pool for twenty years or so, one has about reached the maximum salary regardless of sex; at that point, women's salaries are only about three-quarters the amount of men's.

A third factor which may contribute to the difference is that proportionately more women than men work part-time; and since a part-time employee receives a smaller salary than an otherwise comparable full-time employee, this would contribute to a larger reduction in women's average salary than in men's, a reduction not attributable to overt sex bias. Indeed, if we examine salaries of only those employed full-time, we find that the salaries for both sexes are higher and that the gap between them is narrower. Specifically, the mean salary for men is \$20,500 as compared with \$16,000 for women, a difference of \$4,500 which is somewhat less than the \$5,200 gap for all employed bioscientists. Still, the separation between salaries for the two sexes is quite large, as mean salaries for women still are less than 80 percent of mean salaries for men.

Illustration 3. Mean Salaries, Bioscientists, by Employment Status and Sex

	Male	Female	Difference
Total, Employed Bioscientists	\$19,900	\$14,700	\$5,200
Full-Time Employed	20,500	16,000	4,500

Source: SDSE File.

A fourth factor that may contribute to the difference between the salaries of men and women scientists is a difference in productivity, measured by number or rate of publication of scientific papers. Recent studies show a small but significant difference in productivity of men and women scientists (see notes 7-9).

The available data do not adequately explain why women scientists on the average publish fewer articles than men, but some correlations are suggestive. For the total sample of 499 people studied by Cole and Cole (7), there was only a very small negative correlation between being female and being employed in a high-prestige institution ($r = -.07$). For the 3 fields comprising the sample, however, there were some differences: for chemistry $r = -.27$, for psychology $r = -.23$, and for biology $r = .06$ (the statistical significance of these numbers was not given). That is, in biology women do slightly better than men, but women chemists and psychologists are less likely than their male colleagues (matched by graduate institution and year of degree) to be hired by high-prestige universities. Since these universities provide more resources and incentives for doing research and publishing, their young faculty members are more likely to be productive. In another study of women chemists who received their Ph.D.'s between 1955 and 1961, Reskin (8) found a correlation between productivity and employment in a regular (i.e. tenure-track) faculty position. Yet women were employed more often than men in non-tenure-earning positions, such as research associates (62% of the women vs. 37% of the men). For the men there was no significant correlation between initial rank and subsequent productivity; they were automatically in the contest for success.

We have no data--only subjective impressions--about other factors that may have influenced the productivity of women faculty members. Possible factors include prior socialization to help instead of compete, to be modest about their achievements, and to accept heavy teaching commitments that reduce their time for research. The available data do suggest that success brings further success. Early and continuing publication reaps rewards in the form of citations of the published papers by other scientists. For both men and women chemists, Astin (9) found a

correlation between citations of a person's early publications and continuing productivity. It is possible that citations provide a reward that helps maintain productivity. Unmeasured in these counts of citations are the rewards from responses to papers presented at meetings (again success breeds success) and from informal discussions with colleagues. These informal networks have traditionally been less available to women than to men. (For example, consider Rosalind Franklin's environment at King's College, where the men had their morning coffee-break in a room that was off-limits for women.)

In summary, data show that some a priori possible explanations for differences in the salaries and status of men and women scientists are not valid. However, productivity differences, measured in numbers of publications, do exist. Although the organization of the scientific community has been examined, relatively little is known about attitudes and covert discrimination that may influence productivity and the assignment of rewards within the scientific world.

Footnotes

1. Brackett, Anna C. 1889. "Maria Mitchell." Century 16: 954.
2. THE PROJECT ON THE STATUS AND EDUCATION OF WOMEN of the Association of American Colleges provides a clearinghouse of information concerning women in education, and works with institutions, government agencies, and other associations and programs affecting women in higher education. The project is funded by Carnegie Corporation of New York. The address of the Project is Association of American Colleges, 1818 R Street NW, Washington, D.C. 20009.
3. Free copies of the report are available from the National Center for Education Statistics, 400 Maryland Ave. SW, Washington, D.C. 20202.
4. The report also provides statistical information comparing the occupational status, educational attainment, and work experience of women and men, as well as data on the earnings differential between whites and minority group members. Single copies of The Earnings Gap are available free from the Women's Bureau, Employment Standards Administration, Department of Labor, Washington, D.C. 20210.
5. Bowden, George E., III. August 1977. "Analysis of Sex Differentials Among Ph.D.--Holding Bioscientists." Resource Analysis Memo No. 19, Division of Resource Analysis, National Institutes of Health, Bethesda, Md. Other recent data on salaries of men and women scientists will be found in a 1978 volume of the Annals of the New York Academy of Sciences (in press).
6. Bowden, George E., III. May 1975. "Analysis of Sex Differentials Among Ph.D.-Holding Bioscientists: Salary, Academic Rank, and Predoctoral Awards." Resources Analysis Memo No. 16, Division of

Resource Analysis, Office of Associate Director for Program Planning and Evaluation, National Institutes of Health.

7. Cole, Jonathan R., and Cole, Stephen. 1973. Social Stratification in Science. Chicago: The University of Chicago Press.
8. Reskin, Barbara F. 1977. "Scientific Productivity and the Reward Structure of Science." American Sociological Review 42 (June): 491-504.
9. Astin, Helen S. 1978. "Factors Affecting Women's Scholarly Productivity." In Women: A Challenge to Higher Education, Helen S. Astin and Werner Hirsch, editors. New York: Praeger, in press.

Satisfaction: Some Examples

The following articles illustrate the third kind of reward, personal satisfaction. The first article is an enthusiastic statement of motives by a chemist, Agnes Saunders Riley. The remainder are biographies selected for their diversity: Beatrix Potter, Jocelyn Bell Burnell and Florence Sabin. For each, consider the motivations expressed and implied, the rewards received, if any, and the consequences of being rewarded or not on the individual's career in science. The full biography of Florence Sabin is included to provide a comprehensive look at the life of a successful scientist. Note in particular the different activities Sabin undertook as a professional scientist, the progression of her interests as one research result prompted new investigations, and her continued productivity. Additional examples of the lives of women scientists can be found in the books listed in the Bibliography of Biographies at the end of this section.

WHY BE A CHEMIST?

by Agnes Saunders Riley

From a talk given in January 1940 at the annual banquet of the Oxygen Chapter (University of Washington) of the American Chemical Society (ACS); published in the Iotan Newsletter (May 1973).

There are at least four big reasons why we all want to be chemists.

First of all, be a chemist because it is FUN to be a chemist. Learn the subject for the subject, for the sheer love of it. Is there any satisfaction greater than that which comes from a problem solved, a determination that checks, a new compound prepared, or even one tiny bit contributed to an understanding knowledge of chemistry? Is there anywhere a finer fellowship between coworkers than that which exists among chemists in the ACS or societies like this? Is there anywhere a more cordial relationship than that which exists between chemistry teachers and students or between fellow students? It is fun!

Secondly, be a chemist because it is EXCITING to be a chemist. The day has passed when man could discover but not create, select but not invent. If alchemy is defined in the larger sense as the conversion of worthless material into valuable material, then modern chemistry has achieved alchemy. I call it alchemy, don't you, when one of our best perfumes is made from castor oil and phosgene, and we wear "silk" stockings made from coal, air, and water!

Chemistry is constantly increasing on ever widening fronts. The press only this week carried stories as widely different as the synthesis of vitamin K, the preparation of a satisfactory substitute for egg albumin from soy beans, and the discovery of a new particle in nuclear chemistry.

It is exciting to belong to a field so alive and so growing, but it is to the future we should look.

Thirdly, be a chemist because there is so much TO BE DISCOVERED AND DONE. A famous scientist recently said the next 40 years will see more discoveries than the past 400. What are some of the things needing to be done? Chemistry has found sulfanilamide, but many other specific drugs are needed in medicine's fight against disease. Insulin has been synthesized, but many other body principles are needed. How confused is our present knowledge of the molecular structure of proteins! Since health is dependent upon complex chemical reactions in the body, there is an open field here.

The World's Fair had a marvelous exhibit showing plants without soil, in chemical solutions, but how little we know of photosynthesis, whereby nature builds plant tissue from H_2O and CO_2 . As a matter of fact, chemical synthesis has yet to produce any of the major products of plant and animal life upon which man is dependent for food. The laws of heredity have been worked out for plants and animals, but nothing is known of the chemical composition of the heredity- and character-determining genes.*

Are you interested in the theoretical aspect? Then we need a theory explaining the relation between odor and chemical composition. Why, for example, does the shift of a bromine atom from the para to the meta position make a certain tear gas into a perfume? The scientists studying nuclear chemistry and crystal structure have not yet found the ultimate answers to the Greek philosophers' questions, "What is the nature of matter? What is the force holding matter together?"

Finally, be a chemist because of the SPIRIT OF CHEMISTRY. Like the other sciences, it has set before us a standard of integrity and an objective method of dealing with widely different problems. Love of truth, joy in its discovery, willingness to spend self in creative effort, recognition of the order, beauty, and harmony of the universe. These are the characteristics of great chemists whom we seek to follow.

Three men were cutting stone. "I am cutting a block," said the first. "I am earning five dollars," said the second. "I am building a cathedral," said the third. We may have to be analyzers of compounds and cut many blocks. We may find a congenial job in chemistry and earn five dollars.

*The articles on the discovery of the structure of DNA (Section 2) provide more current information on this problem (ed.).

But is there any reason why we should not help build a cathedral by taking our place along side the men and women who have made chemistry a real creative force?

Why be a chemist? Now, honestly, would you be anything else?

THE SECRET LIFE OF BEATRIX POTTER

by Naomi Gilpatrick

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Waiting for the director of the Royal Botanic Gardens to appear, the young woman stood, silent and watchful, in the shadow of the trees. Beatrix Potter was half-inclined to flee. Her theories on symbiosis, which she had written up in a paper on the germination of spores, went beyond what was known in 1896, and she needed a sponsor. But the director of the Gardens had no time for her or her discoveries. She watched two women assistants at work, and after learning that they were "obliged to wear knickerbockers," she wrote in her journal, using a code alphabet of her own invention, that the director "may be a misogynist."

Following Beatrix Potter's death in 1943 at the age of seventy-seven, Leslie Linder, an engineer whose leisure-time hobby was collecting Potter drawings, heard that a bundle of pages in keyless code had been found in the farmhouse of the author of The Tale of Peter Rabbit. Challenged, Linder worked on and off for years trying to break the code. He finally succeeded in 1958, but it took him several more years to decipher the diary. Beatrix Potter's Journal, covering the years from 1881 to 1897, was published in 1966, and in it can be found the story of her efforts to present her theories to the scientific authorities of the time, who reacted as though "one must not speak to them."

She would have liked to discuss her growing portfolio of fungus and lichen drawings with some of the scientists at the Botanic Gardens. She had questions to ask--small, moot points that weren't touched upon in any of the books she had consulted. She had never gone to school; instead, she had been tutored by governesses hired to teach her French and German. She had made good use of this by consulting natural history books in both these languages, especially those by Louis Pasteur and Oskar Brefeld. Her own observations, made not only in her third-floor study but also on frequent holidays to seacoast towns with her father, a leisure-class photographer, had brought her, in her twenties, to the forefront of what was known about lichens and fungi.

In her father's company, she found that Sir William Flower, who was associated with the Museum of Natural History at South Kensington, would acknowledge her, but at the museum he would walk past as if she weren't there. One Sunday, she attended a gathering at the home of the local police magistrate, hoping to meet and confer with Sir William there. But she was sitting in the darkened alcove of the room when he entered and he gave no sign of recognizing her. That night she wrote in her journal, "I wonder if people know the pleasure they may give a person by a little notice."

Trudging the fields hadn't equipped her for the small talk necessary to initiate a conversation. Too long had she silently assisted with the camera while her voluble father carried on animated conversations with the great men he photographed: John Bright, a member of Parliament; Prime Minister Gladstone; and the painter Sir John Millais, who said, in her presence, that the length of her nose and upper lip spoiled her face. No wonder she couldn't lift it from the museum's fossil cases to get the answers she needed.

She wrote in her journal that the museum "is the quietest place I know--and the most awkward. They have reached such a pitch of propriety that one cannot ask the simplest question." Her frustration mounted as she studied the labels on insects. "I [am] worked into indignation about that august Institution. The clerks seem to be all gentlemen and one must not speak to them."

Holiday trips with her parents, which allowed her to explore quarries for fossils, made her feel that "with opportunity the world is very interesting." At the age of twenty-eight she observed a falling meteor, but decided that she wouldn't start to study the stars, with forty thousand named and classified fungi yet to be observed. By then, she had found and painted more than two hundred species.

During her teens and twenties, Beatrix Potter's search for fungi took her out of doors each day that weather permitted. Nothing discouraged her, not even the average of seventy insect bites ("suspect spiders") that she received on each search. What mattered was climbing over a hedge, going into a wood, and finding "a paradise of funguses." Even the danger of getting lost in the depths of a black fir forest did not deter her from plunging through the bracken until she found the yellow Peziza in the moss or the gigantic Cortinarius.

Her search for fungi was unremitting. The emotion she recorded upon coming across a new species for the first time was "joy of joys." She went deeper and deeper into "the green fogginess and tangle," pausing at what seemed to her "an ideal heavenly dream. . . . The fungus starred the ground apparently in thousands."

Beatrix Potter, before she turned to writing for children, was the first person in England to affirm that the colorful patches growing on trees, fences, tombs, and rocks were actually a merging of two discrete plants--an alga and a fungus--to make a third kind of plant, a lichen, which in function and longevity was different from either of the two originators.

In 1867 the Swiss botanist Simon Schwendener was the first to describe lichens as dual organisms, but British botanists rejected this announcement. In 1874, when Beatrix Potter was eight years old and already assiduously painting and labeling caterpillars in her sketchbook, the English botanist J. M. Crombie came out with a sardonic dismissal of Schwendener, who, he wrote, was advancing an "unnatural union between a captive Algal damsel and a tyrant Fungal master." In an article ten years

later in the Journal of the Linnean Society, Crombie continued to denounce the "alga-lichen hypothesis."

In 1879 a German botanist, Heinrich de Bary, coined the word symbiosis to designate a sharing of physiological mechanisms by dissimilar organisms. By 1894, Beatrix Potter was sufficiently advanced in her own researches to take a lively interest in Johannes Reinke's findings in that year that symbiosis was the life force responsible for generating a new departure in plant life: the lichen.

But she could find no one at either the Kensington Museum or at the Royal Botanic Gardens at Kew who would concur with her own microscopic findings about the processes integrating fungi and algae into lichens. It took a great deal of tactful talking on her part to bring the assistant at the Royal Herbarium halfway around to "believe my new-thing, Lichens," she wrote in her journal pinpointing by a date, November 27, 1896, when her "idea of the lichens" first came to her. Her observations of symbiosis convinced her that the fungal partner was not a parasite, but a contributing member of a pair Reinke called "two consorts."

To prove this theory to the authorities at Kew, Potter drew on the know-how of her uncle, Sir Henry E. Roscoe, a chemist who had been knighted two years earlier for his scientific contributions. He suggested ways of testing the algae for chlorophyll lines with a spectroscope.

This research led Potter to the conclusion that, far from the parasite that some German and Swiss botanists held the fungal partner to be (authorities in England doubted it was a partner in any sense), the fungus contributed minerals extracted from air and water needed by the alga. Alone, the fungus had no chlorophyll for making its own food. The alga had green chlorophyll, which traps light, converting it to chemical energy for turning carbon dioxide into sugar. While the alga manufactured the sugar, starch, and oxygen that nourished itself and the fungal strands, what triggered the chlorophyll to act was water stored in the sponge-like fungus. The alga had a modicum of water, but it was excited to vigorous photosynthetic activity by contact with the fungus, whose rapid uptake of liquid water was by imbibation. After a rain, some fungal components hold up to 35 times their dry weight in water. They have the capacity to absorb water from fog, dew, and the air. The photosynthetic activity of the algal component is stimulated by the fungal partner's surplus water, which contains dissolved mineral salts, chemicals, and carbon dioxide--all elements needed by the alga to make vitamins for both.

It was through her uncle, Sir Henry, that Beatrix Potter finally got the opportunity to meet the director of the Royal Botanic Gardens, Sir William Turner Thiselton-Dyer. Sir Henry, while paying a sick call on her father, looked at her portfolio of fungus paintings and, impressed, offered to take her to see Thiselton-Dyer.

Once at the Gardens, she noted with astonishment that her uncle, in introducing her to five different officials in turn, had assumed an engaging simper. The assistant director, D. Morris, was sorting crumpled papers

containing gums from Arabia. "I know nothing about fungi," he told them brusquely. "I am exclusively tropical."

They left him to his specialty and went across Kew Green to the herbarium, pausing to see W. B. Hemsley, but he was too intent on stacks of dried papers to give any time. One assistant, George Massee, did look at her drawings with pleasure. But J. G. Baker, the librarian, a thin man who seemed himself to have "been dried . . . under a press," took refuge in the silence of a deep bow.

At last they were in the office of Thiselton-Dyer. With a cigarette in one hand and an alert manner, he turned the pages of her portfolio. A look of surprise came into his face. "I'll be glad to renew your student ticket to admit you to the gardens and the library," he told her, and then, turning to her uncle to talk about politics, he did not address her again. She had built herself up to meet each in turn and now she was tired. She felt that Thiselton-Dyer thought she was too young to be given further consideration.

She had brought her drawings to the director to show him that while the fungal strands enclosed the green algal cells securely to draw in carbohydrates, vitamins, and oxygen, the water from swollen fungal hyphae was crucial for this process and yielded mineral nutrients to the alga partner. The fast-growing hyphae cling to bare rocks, extracting ions and minerals, anchoring the algae while the fungal walls shield the algae from too intense light. She pointed out that the antibiotic qualities of the substances adhering to the fungal strands protect lichens from pathogenic attacks, insuring a greater longevity to the composite plant than either component could enjoy alone. But Thiselton-Dyer was indifferent, and shrugged away her paintings as too artistic to meet scientific criteria.

Three weeks later she went back to Kew and sought out George Massee, the assistant who had seemed interested in her paintings. He showed her the fungi he was growing under glass covers, and as he talked she detected that his ideas were "not founded on very sufficient evidence." It was clear to her that the man had "passed several stages of development into a fungus himself."

From there, she went to the Museum of Natural History to identify some specimens and "was further edified by the slowness of the officials." It wasn't that they weren't kind. What bothered her was that "they do not seem to be half-sharp." B. Waterhouse, in charge of beetles (one of her subjects in painting), looked so much like a frog that she had once kept as a pet that she couldn't bring herself to discuss beetles with him. Sir William Flower passed her and remarked pleasantly on her bonnet. What is his subject besides ladies' bonnets? she wondered later in her journal. Her day was saved by meeting a Mr. R. Pocock, who liked her painting of a male crab spider and talked about spiders with her. She didn't tell him she thought the spider case was "an extreme example of museum labelling run mad." In her journal that night she wrote, grimly, "They are almost too much specialists; they really seem less well informed than an ordinary person on any subject outside their own."

She continued to develop theories about fungi. Those that spring up on fresh manure in the summer, she decided, must have another source to take them over the winter months. She concluded that all higher fungi must have a mold. This is how, she reasoned, the Chlorospenium gets from log to log "without cups to spore." Her uncle advised her to write a "fishing" letter to Massee at Kew ostensibly asking for a book, but actually trying to find out how much the authorities knew about spore culture.

The answer came two days later on a postcard: "Botanische Untersuchungen uber Schimmelpilze, by Oskar Brefeld." Disgusted, the next day she drove to Kew in her pony cart and faced Massee with a direct question, Had Dr. Brefeld actually "got the mushroom mould"? Massee said, "No." He was looking at her vaguely through his glasses. It took her only two minutes to discover that Massee "knew very little about it." The discussion about her mushroom slips became heated. She wrote in recollection of the encounter: "I am afraid I contradicted him badly."

Her uncle exulted, "No one but you and Dr. Brefeld have grown spores." Sir Henry urged her to write a paper on her theory. When she showed her father the first draft of her paper, he dismayed her by going through it with a proprietary pencil. After listening to his remarks about her grammar, she wished she had never shown it to him.

Her ideas about lichens generated "another idea about hybrids." She looked forward to delivering the paper to Thiselton-Dyer and to discussing it with him. She went so far as to take the train to Kew, but when she reached the gates, shyness overcame her. She watched through the window for fifteen minutes before going in; then she sat in an outer room and tried to distract herself by feeling the patterns of the chair legs. When an old gentleman came to the door to look at her with curiosity, she "incontinently fled."

The following Monday, she returned to Kew. Although it was a long wait, this time she stayed until an impatient and cynical Thiselton-Dyer bounced in, telling her that her opinions were mares' nests, that he hadn't time to look at her drawings, and that she ought to go to Cambridge University. "It will all be in the books in ten years," she told him. She left at that point, because she found herself beginning to laugh.

In the herbarium, she found that Massee had become a covert to her theory. But in listening to him, she had to conclude that he didn't have a very clear concept of it. She found herself amazed at "how botanists have niggled at a few isolated species and not in the least seen the broad bearings of it." As it was, Massee urged caution and continued experiments.

A week later she stopped by to visit her uncle. He had finished his breakfast and was sitting back on the sofa, smoking. He told her he had received a "rude and stupid" letter from Thiselton-Dyer.

"Maybe there's more to what you are working on than even I realized. Do you think you could explain to me in detail and very carefully just what it is you are advancing?"

She was delighted. Pulling paper and pencil toward her, she drew and sketched while she explained. Sir Henry listened intently, making a great effort to understand, asking at each step, "Are you quite sure?" Annoyed at the slight given to one under his patronage, he told her that no stone would go unturned until her paper went to the most prestigious scientific group in London, the Linnean Society.

Thiselton-Dyer's rude letter acted on him as a spur. Sir Henry's single-minded absorption in showing up the martinet in charge of the Royal Botanic Gardens made him forget a touch of gout, and he soon sent for Beatrix again and questioned her closely. He pointed out places in her paper that she had to clarify.

"You've seen," he asked her, "that most fungi can form a product that looks like yeast?"

"Yes. And since telling you that, I've found Brefeld agrees."

"Does Brefeld have it all?" he asked searchingly.

"He has plenty of facts and plenty of experiments, but he doesn't know which are of interest and which are deadwood. He has theories, too; but they don't piece on to the experiments."

"And Pasteur?"

"He stopped experimenting after he tried and failed to get yeast from Penicillium, but he took it for a single species, whereas there are hundreds."

That night, stimulated by her discussion with her uncle, she worked on her paper until midnight, even though it was Christmas. To have spent the whole afternoon discussing lichens with her tireless, white-haired uncle and the whole evening writing about them was to Beatrix Potter, the lichenologist, the perfect way to spend her thirtieth Christmas. The next morning she was up bright and early, paper in hand, going across the snow to her uncle's. She thought it would take but a minute for him to look over her paper, but he had gotten his second wind. With a well-sharpened pencil, he pointed out places in her paper where more references "and putting together" were needed. As the morning wore on, she realized that she had more writing to do before braving "the misogynist."

Her uncle said, "It's because you're an outsider that you have to get this right," and lent her some more of Pasteur's works.

Once home, she saw what he was trying to do for her. He didn't want her to throw in pieces haphazardly, as Brefeld did, but rather, to achieve the sense of unity that Pasteur had. She treasured his pencil marks, which showed someone was interested. Her mother, going out in her carriage every day on a round of social calls, and her father, with his photography of the great and famous, found her unremitting collecting of

fungi a grubby and inexplicable preoccupation. "If you have any reason for doing it," her mother told her, "I don't want to hear it."

Beatrix Potter was beginning to have serious second thoughts about pitting her amateur know-how against the big guns at the university and the London Linnean Society. In retrospect, her long years of sharing data with herbalists met on the road or with Charlie Macintosh, the postman, seemed better. When he had called at her invitation five years before, Macintosh told her he was studying freshwater algae. At the time, she wrote that as he looked over her drawings, his accuracy in "minute botanical points gave me infinitely more pleasure than that of critics who assume more, and know less." Now, five years later as she prepared to go to London to face the experts, she wrote in her journal, "I prefer the sagacity of the man in the street."

Coming to her uncle's with the final draft of her paper on the last day of January, she was in a "state of abject fright." He read the paper intently, suggesting no further changes. "Do you think Professor Ward will have the patience to read it through?" she asked, facing up to the prospect that the opinion of Henry Marshall Ward, professor of botany at Cambridge, might be unfavorable.

Her uncle, however, urged her to present it. On April 1, 1897, the paper, entitled "On the Germination of the Spores of Agaricineae," by Helen B. Potter, was read at a meeting of the Linnean Society of London. It was read by George Massee; Beatrix was not present, for only men were allowed to attend the meetings.

The paper told of her experiments in trying to get spores to propagate apart from their natural habitat. Before the paper could be published, however, she withdrew it because she wanted to conduct further experiments. Her extensive reading, as well as her talks with authorities at the museum and herbarium, made her aware that she was the first Britisher to break ground in the germination of spores, and the first Britisher to point out the positive contributions of fungi to algae in the lichen union. She wrote in her journal regarding chlorophyll and symbiosis, "I don't think anyone else is at it."

It helps one to understand how hotly that lichen controversy raged by realizing that even today the question is unresolved. While most authorities unquestioningly affirm the reciprocal benefits enjoyed, in some degree, by the two components making up the lichen, others assert that, in most cases, the relationship is parasitic. A few claim that there is no transforming exchange in lichens, since the alga can exist independently if separated. The controversy persists, largely because of the many types of lichenized unions. Most authorities agree that lichenology has more unexplored horizons than any other field of plant study.

Now that her theory had reached British scholars, Beatrix Potter hoped that if any of those botanists present at the meeting mentioned it in their books, they would acknowledge her. But the prospect of devoting another two decades of her life to achieve a second footnote in the botany

books was no longer inviting to her. However, what prevented Beatrix Potter's receiving even delayed appreciation for her studies was that at her death in 1943, or shortly thereafter, many of her voluminous notes were burned. Among these was her paper on spores.

She gave up her studies of spores two years after she had asked for the return of her paper because the time and energy she had invested in painting fungi and lichens now went into writing and illustrating children's books. She found the response from child readers much more heartening.

The children in her former governess's family had always delighted in the illustrated letters she wrote to them. When in 1890, seven years before her lichen paper was finished, she received her first check for six Christmas card designs, she was so pleased she gave her rabbit model, Benjamin Bouncer, a cupful of hemp seeds as a reward. When she tried to use him as a model the next morning she found him partially intoxicated and unmanageable.

Beatrix began to wonder if a whole book about a rabbit would be equally well received, and she sent her book to six publishers, to no avail. The individualist who had gone past an impasse at the museum to reach a scientific audience at the Linnean Society, in a similar way went past the rejection of publishers by paying a printer named Strangeways to print her first book for children. When it appeared, the firm of Frederick Warne, Ltd., informed her that they would issue it under their imprint if she would redo the pictures, this time using color. So with the same preciseness she had used to differentiate fungi, she now set herself to learning and perfecting the technique of creating color illustrations for small books.

The Tale of Peter Rabbit has been on the list of best-selling children's books every year since its appearance seventy years ago. It ushered in a new career for its author. She put aside the detailed watercolors of fungi (by 1901 she had completed 270--all of which are now in the Armit Library, Ambleside, England), intending to return to her research and drawings of spore development after the interest in Peter Rabbit died down. But the clamor of children and of booksellers led her to invest her energy in producing book after book, twenty-five in all. The spore paper was never published, nagging her with the sense of a goal not attained.

One easily detects this flavor of frustration in Beatrix Potter's story-books: the sense of being kept from a desired goal. The tailor of Gloucester can't work on the wedding garment because the cat has stolen the twist of thread he needs for his buttonhole. Jeremy Fisher, the frog, can't treat his friends, the tortoise and salamander, to a minnow dinner because he has been swallowed by a trout. Peter Rabbit can't range the garden at will because of the upraised rake of pursuing gardener, McGregor. Squirrel Nutkin, with Beatrix-Potter-like effrontery, dared to twit the owl--who represented the Establishment on the island--and lost his tail. Tom Kitten was stopped in his exploring by two rats, who tied him up and wrapped him in dough to make a pudding of him. Jemima Puddle-duck found a "safe" place to sit on her eggs as guest of a saue

fox who, like Sir William Flower with Beatrix Potter, complimented Jemima on her bonnet.

As the royalties from her books increased, Beatrix was able to buy a farm at Sawrey. In 1913, at the age of forty-seven, she married William Heelis, a solicitor who advised her in buying land in the Lake District. With him she enjoyed thirty years of happiness. Little is known of these years, however, for she had ceased writing in her journal on the day in 1897 when she brought the final draft of her paper on spores to her uncle.

After her marriage she became a sheep raiser, with a summer population of 1,800 Herdwick sheep and 70 head of cattle on her ten farms. Many of these she brought to fairs in the autumn, where other breeders sought her advice. So skilled was she in raising the hardy blue sheep that the farmers of Sawrey elected her president of the Herdwick Breeders' Association, the first time a woman had ever held that post. Into one of her longest books, The Fairy Caravan, she put her experiences of rescuing sheep who had gotten cragged or buried under snowdrifts, flavoring the book with shepherds' talk.

In her seventies, when she was too old to climb the steep mountain crags, she walked the windswept hills in her mind's eye, knowing by heart every path, boulder, overhang, and stream. So that none of this countryside would be urbanized, she used her book royalties to buy farm after farm, willing 4,049 acres and fifteen cottages to the National Trust as a place of sanctuary where no hunter would rout the sheep and birds or chase Peter Rabbit. In peace then, in 1943 at the age of seventy-seven, she could close her eyes upon it all.

DISCOVERY OF PULSARS:
A GRADUATE STUDENT'S STORY

by Nicholas Wade

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Hombury St. Mary, Surrey. Perhaps the most dramatic scientific event of the last decade was the discovery in 1967 of the celestial objects known as pulsating radio stars or pulsars. The radioastronomy group at Cambridge, England, announced their detection of a wholly novel class of stars which beamed out pulses of radio waves with extraordinary rapidity and precision. The pulsars were soon determined to be neutron stars, the long postulated embers of stellar evolution that astronomers had assumed were too faint ever to be detectable from Earth. And the public who followed these events enjoyed the frisson of learning that the Cambridge astronomers had for a time considered the signals might originate from another civilization, in token of which they had nicknamed the pulsars LGM stars, for "little green men."

Just how pulsars came to be discovered is a historically important case study in serendipity. The manner of the discovery is also of topical interest because of a dispute that has blown up over the award of last year's Nobel prize for physics. For the first time, the prize went to astronomers, and the Nobel committee honored Martin Ryle, leader of the Cambridge radio-astronomy team, and his colleague Anthony Hewish, under whose supervision pulsars were discovered (see Science, 15 November 1974).

Hewish's citation was "for his decisive role in the discovery of pulsars," the qualification being there because, as Hewish has always acknowledged, the first finder of pulsars was his graduate student, Jocelyn Bell, now Burnell. Nobel prize committees have a reputation for doing their homework thoroughly, and if anyone thought the credit had been wrongly distributed, they kept their doubts to themselves, at least until recently. This March, after a lecture at McGill University, Montreal, English theoretical astronomer Fred Hoyle was reported to have described the award to Hewish as a scandal. According to The Times of London, Hoyle declared that Burnell's finding had been kept secret for six months while her directors "were busily pinching the discovery from the girl, or that was what it amounted to." But the victim of the alleged conspiracy disagreed. "It's a bit preposterous and he has overstated the case so as to be incorrect," she told The Times's reporter.

There the matter might have rested, but a few days later Hoyle wrote from Rice University, Houston, where he was teaching at the time, to say that his views had been "only crudely represented." While not in fact repudiating any of the remarks attributed to him in the initial article, Hoyle

proceeded to lay out his case more precisely. The actual discovery of pulsars, he said in his letter to The Times (published 8 April), seemed to have taken place in the two-month period up to September 1967, but the finding was not published until February 1968, by which time it had accrued five authors including Hewish and Bell, together with four others who were cited for help in the discussions that led to the report. The publication consisted of two parts: the detection of the first pulsar, and a follow-up investigation. The second part of the process was guided by Hewish, but could have been done equally well by other observatories, Hoyle opined. The discovery of the signals by Burnell, and her finding that the source of the signals changed position with the stars, was what constituted the crucial step: "Once this step had been taken, nothing that happened from there on could have made any difference to the eventual outcome." Hoyle continued:

There has been a tendency to misunderstand the magnitude of Miss Bell's achievement, because it sounds so simple--just to search and search through a great mass of records. The achievement came from a willingness to contemplate as a serious possibility a phenomenon that all past experience suggested was impossible. I have to go back in my mind to the discovery of radioactivity by Henri Becquerel for a comparable example of a scientific bolt from the blue.

I would add that my criticism of the Nobel award was directed against the awards committee itself, not against Professor Hewish. It seems clear that the committee did not bother itself to understand what happened in this case.

It says nothing as to the merits or demerits of Hoyle's argument to note that his letter is part of a long series of dissensions which have made British astronomy a game quite unfit for children. The dispute has led, in one way or another, to Hoyle's resignation from his Cambridge Institute of Theoretical Astronomy (see Science, 2 June 1972) and to the resignation of Margaret Burbidge, now at the University of California, San Diego, from the directorship of the Royal Greenwich Observatory (see Science, 30 November 1973).

Hoyle's letter presented Hewish with the virtually no-win choice between saying nothing, thus seeming to have nothing to say, and defending his role in the discovery at Burnell's expense, with the risk of appearing somewhat ungallant. His decision was to reply with restraint (11 April), saying in effect that Burnell had been using his telescope, under his instructions, to make a sky survey which he had initiated. There were several problematical features of the pulsed source--a slight variability in its time of appearance, the possibility that it was of human or alien origin--that had to be resolved under his direction. Although the source was first discovered in August, it was not until January 1968, the month before publication, that the necessary tests could be completed. Hewish concluded his account (which he had checked first with Burnell) by saying her performance as a graduate student was exemplary, but that

it was "unjust to the painstaking efforts of others who have continued the sky survey to suggest that their work would have overlooked the first pulsars." Hewish gives a fuller account of his view of the discovery in his Nobel lecture, published in Science (13 June 1975).

In preface to Jocelyn Burnell's side of the story, it should be said that she has no disagreement with Hewish. She thinks that he deserves the prize, that she has been fairly treated, and that if he got more of the credit than she did, he also took more of a risk. Discovery of pulsars was an exciting event in her life--she was 24 at the time--but she is in no way mesmerized by it. She is working at present with a small team analyzing the data from a British X-ray satellite. Her laboratory, a rural outpost of London University, is situated in Hombury St. Mary, a small hilltop village that overlooks the Surrey downs.

Burnell grew up almost literally in the shadow of astronomy, her father being the architect for the observatory at Armagh in Northern Ireland. From early on she wanted to be an astronomer but had to surmount the hurdles of a poor science education in high school--"The staff were not very well gened up on physics"--and a tough university course at Glasgow--"I was the only woman left in the physics class by the end of the first year."

With a bachelor's degree in physics, she was accepted as a Ph.D. student by the radioastronomy group at Cambridge in 1965. Graduate students are often jokingly referred to as slave labor. Burnell spent her first two years constructing the radio-telescope she was to operate and, though slight in stature, she could swing a 20-pound sledgehammer by the time she left Cambridge.

The instrument, known as the $4\frac{1}{2}$ acre telescope because of the area covered by its 2000 dipoles, was designed to survey radio galaxies and to measure their angular diameter from the way the signals "twinkled"--an effect Hewish had discovered to be caused by the solar wind. The special design features that enabled the telescope to detect the twinkling were, as it happened, just what were needed to pick up the rapidly varying signals from the as yet unknown and unsought for pulsars.

Having helped build the telescope, it was Burnell's task singlehandedly to operate it and analyze its data until she had enough material for a thesis. The analysis was not much less arduous than the construction work. The telescope churned out 96 feet of three-track paper every day. It took four days to cover the sky, so Burnell had about 400 feet of paper chart to analyze for each complete coverage of the sky. Her job was to scan the chart by eye, mapping the signals that were true twinkling radio sources and discarding those that came from man-made sources of interference such as French television, aircraft altimeters, and pirate radio stations.

The temptation to cut corners on the analysis must have been considerable. The telescope was switched on in July 1967. By October, Burnell was 1000 feet of chart behind and by the end of November she was lagging

a third of a mile. It was in October that she discovered the pulsar. Its signal, which she describes as "a bit of scruff," occupied about half an inch of the 400 feet of chart. "The first thing I noticed was that sometimes within the record there were signals that I could not quite classify. They weren't either twinkling or man-made interference. I began to remember that I had seen this partiucular bit of scruff before, and from the same part of the sky. It seemed to be keeping pace with 23 hours, 56 minutes, i.e., with the rotation of the stars."

Though much remained to be done, the nub of the discovery lay in that single instant of recognition. She remembered she had seen the same "bit of scruff" at roughly the same time of day in previous charts, and she found on measurement that it was keeping sidereal, not terrestrial, time. (The sidereal day, because of the earth's revolution about the sun, is 4 minutes shorter than the terrestrial day.) In Burnell's words: "When it clicked that I had seen it before I did a double click, I remembered I had seen it from the same part of the sky before. This bit of scruff was something I didn't completely understand--my brain just hung on to it and I remembered I had seen it before."

Looking back through the records, she found the first time the scruff had appeared was on 6 August. She discussed the signals with Hewish, and they jointly decided to look at them on the observatory's fast recorder, then occupied with another task, so as to get a clearer picture of the signal's structure. The recorder became available in mid-November, and Burnell went out to the telescope at the time of day the source was passing through the beam. But for several weeks nothing happened. The signal, at all times variable, failed to show at all. "Hewish was thinking at that stage that it was a flare star and that we had missed it," says Burnell. "Finally one day I managed to catch it, and I got a series of pulses coming out of the recorder. They were almost exactly 1-1/3 seconds apart. That is a very sort of man-made period. Tony Hewish had left the recording to me. I phoned him up to tell him about the pulses and he said, 'Oh that settles it, it must be man-made.'"

Hewish came out to the observatory the next day to watch Burnell make another fast recording. The signal was quite strong that day and she was able to produce a nice train of pulses for her supervisor's satisfaction. Hewish then went through the records and confirmed that the source was keeping sidereal time.

"We had terrible trouble trying to sort out that conundrum," Burnell recalls. The problem was that the fastest variable star then known had a period of one-third of a day, and no one could conceive of a star with a period of 1-1/3 seconds. But the source couldn't be man-made either because it reappeared with each revolution of the stars, not with each rotation of the earth. Could it be radar signals bouncing off the moon, or a satellite in a peculiar orbit? That didn't fit. Burnell and Hewish then realized that the only people on Earth who keep the 23-hour, 56-minute schedules of sidereal time were other astronomers.

"Hewish wrote round to all other observatories asking them if they had had any such program going since October. All wrote back saying, 'No we haven't.'" The next idea was the little green men thesis. "Although they don't take it very seriously," remarks Burnell, "radioastronomers are aware that they would probably be the first people to come into contact with other civilizations. So Hewish timed the pulses to see if there was a Doppler shift." The rationale was that the aliens would presumably be resident on a planet, and the planet's orbital movement around its sun would produce a bunching of the pulses as it moved toward the earth and a spacing out of pulses as it moved away.

This moment in the investigation, when the Cambridge radioastronomy group was seriously considering that the pulses might be signals from another civilization, is described with notable terseness and understatement in the logbook that Burnell kept at the time. Her entry for 19 December 1967 refers to a radio "source"--source in quotation marks because Hewish still doubted that it was genuine--but under the remarkably prescient heading "Belisha beacon." A Belisha beacon is the regularly flashing orange globe that warns motorists in England of a pedestrian crossing; Burnell gave the source this private nickname--the rest of the group called it the LGM star--before she had even detected the pulsations on the fast recorder. Her logbook reads as follows:

Belisha beacon. There exists a radio "source" which keeps constant RA. [right ascension] to within $5''$ and constant dec [declination] to $1/2^\circ$, which has been observed over $4\frac{1}{2}$ months. It passes through the beam of the aerial at the same rate and in the same direction as any other source. It consists of pulses at equal intervals (1.3 secs.). . . . This "source" is very close to the Galactic plane ($b'' = 4^\circ$). The one mile telescope [one of Ryle's big radiotelescopes] has had various attempts to find it, but has not seen it so far. We are working on the Doppler shift of the pulses to see whether the source is stationary or moving round a sun. There is no 4C source [the Cambridge catalog of radio sources] with the same coordinates, nor any other source that we know of. . . .

As it happened, Hewish found no Doppler shift except that due to the motion of the earth, which ruled out any planet or little green men or alien communications system. At the same time, or slightly before, Burnell took the step which more or less settled the nature of the "source." She discovered a second "source." The day before she left Cambridge for her Christmas holiday, "I was working in the evening analyzing chart. I saw something which looked remarkably like the bits of scruff we had been working with. This was in a bit of sky that wasn't very easy for the telescope to look at, but there was enough to confirm that there had been scruff.

"That particular bit of sky was due to go through the beam at 1 a.m. It was a very cold night, and the telescope doesn't perform very well in cold weather. I breathed hot air on it, I kicked and swore at it, and I

got it to work for just 5 minutes. It was the right 5 minutes, and at the right setting. The source gave a train of pulses, but with a different period, of about $1\frac{1}{4}$ seconds."

Did she phone Hewish to tell him of the second discovery? "Not at 3 o'clock in the morning, oh no. I dumped the recording on his desk and left for my holiday. I don't think he really believed that one. But he kindly kept the telescope running and the inkwells full of ink while I was away."

Hewish himself did a recording in the middle of the night in mid-January and confirmed the second source of pulses. "That removed the worry about little green men, since there wouldn't be two lots signaling us at different frequencies," says Burnell. "So obviously we were dealing with some sort of very rapid star. I threw up another two sometime in January."

Those were the last two pulsars she discovered because by mid-January it was time to start writing up her Ph.D. thesis, to which pulsars appeared as an appendix. The existence of the stars, which hitherto had been kept a closely guarded secret, was announced by Hewish at a seminar in Cambridge, and to a wider audience by the paper in *Nature* (24 February 1968). Burnell's name was second on the list of authors, followed by those of three other members of the radioastronomy group who helped with various stages of the follow-up study. Following custom, there was no indication of who had contributed what.

Publication caused a sensation, but Burnell was busy trying to get her Ph.D. thesis written up and looking for a job in the south of England, where her fiancé, a government employee, was then working. She joined a group doing gamma ray astronomy at Southampton University and taught there for five years, but had poor scientific data to work with. A year ago she came to the laboratory at Hombury St. Mary, where she works half time--afternoons are spent at home looking after her two-year-old son. Her group has obtained excellent data from their X-ray satellite. Earlier this year they discovered a transient X-ray source with the interesting period of $6\frac{1}{4}$ minutes. Recently Burnell and her team discovered a new transient source near the Crab nebula with a period of about $1\frac{1}{2}$ minutes.

Looking back on pulsars, Burnell feels that the hardest part about the discovery was simply spotting the signals amid each 400 feet of chart paper. A second danger point was that the signals might have been dismissed as man-made interference. She has heard rumors that another radioastronomer in Britain noticed pulsar signals on his charts before her discovery but failed to follow them up. She rejects the implication in Hoyle's letter that she had to pursue her investigation of the anomalous signals in the teeth of terrible opposition, which, she says, was certainly not the case. Yet she seems to have been less ready than her elders to doubt that her "bit of scruff" was a real signal from a real star. "I certainly joined in the business of trying to explain it away as wholeheartedly as everyone else," she says, "I think I probably suspended belief." But, even if suspended, the belief that her "Belisha beacon" was a

genuine star was always there. "My background in astronomy wasn't as good as Hewish's and I didn't appreciate all the risks. He ruled it out as a star--muggins [herself] didn't realize why it could not have been a star. I continued to think it was a star until someone pointed out to me how fast . . . [the rate of pulsation] was."

Should Burnell have received a share of the Nobel prize for pulsars? The question raises some tangled issues. For one thing, there is a general expectation among scientists that the Nobel prize should not be awarded for a single chance discovery, but should go to the individual with a long career of successful contributions. Nobel's will, however, says nothing about careers: he stipulated simply that the award should go to "the person who shall have made the most important discovery or invention within the field of physics."

Pulsars may not now seem as tremendously interesting as when they were less well understood, but they are still sufficiently remarkable objects, many astronomers would say, to merit a Nobel prize for their discoverer, assuming that the prize should be given for a single discovery. But how should the credit for the discovery be distributed? Nuclear physicists employ trained scanners to analyze photographs of particle tracks. No one has suggested that the scanners should receive a Nobel prize when they pick up a new particle. But there are differences between this case and that of pulsars. The discovery of pulsars was neither planned nor predicted: as Hewish says in his Nobel lecture, he stumbled upon them.

Nor does Burnell's contribution seem to everyone to have been as routine as a hired scanner's. Hewish considers that it was. "Jocelyn was a jolly good girl but she was just doing her job. She noticed this source was doing this thing. If she hadn't noticed it, it would have been negligent," he told *Science*. Cornell theoretical astronomer Thomas Gold disagrees. Gold, who first came up with the accepted theoretical explanation of pulsars, points out that Burnell, unlike a photo scanner, understood the basis of what she was doing and, moreover, had not been told to look for pulsars. "She was told to plot scintillating radio sources, but she noted and pursued in her own way a different kind of signal," Gold remarks.

Hewish's point is that he had instructed Burnell to plot all signals on a graph; the genuine radio sources would reappear from day to day, interference signals would not. From this routine, pulsars would emerge automatically. The signal "didn't look very different in the record from anything else we were plotting. It's an absolute certainty that [if Burnell had not done so] someone else would have picked it up," says Hewish.

"I would not say it was 100 percent automatic, but I would think that it is reasonably likely that that telescope would have discovered pulsars sooner or later," replies Burnell. In the early stages of operation, however, the routine for analyzing the recordings had not been fully worked out. The "bit of scruff" looked different both from the scintillating sources and the interference signals she had been told to plot.

Though she cannot remember definitely, she "suspects" that she did not record it on the graph. Asked about this point, Hewish says he does not remember if Burnell plotted the pulsar signals on the graph and would trust her memory of what happened; he does remember, however, that the first time she mentioned the signal she showed him the direct recording from the telescope. Thus it would seem that Burnell's discovery of the repeating nature of the signals was a feat of unaided memory and observation, not the result of following a preset routine; in turning up the pulsars she was going beyond the precise letter of her instructions, even if acting within their general framework.

It may indeed be true that the next graduate student down the line would have made the discovery if she hadn't. Yet with this "what-if" argument, almost every scientist could be stripped of his credit, in that every discovery would have been made by somebody else sooner or later. The historical fact is that it was Burnell, and not another individual, who discovered the pulsar signals.

A prize committee which decides that each of two individuals had made an essential contribution to a discovery may then attempt the exercise of determining whose is the more important. Or, it may think further discrimination is futile and award its prize to both. In the case of pulsars, the Nobel committee took the first course, the Franklin Institute of Philadelphia the second. The institute, which prides itself on the thoroughness of its prize-giving investigations--the process usually takes 1½ to 2 years--awarded its Albert A. Michelson medal to Hewish and Burnell jointly in 1973. The institute declines to discuss the basis of the award, but a prize committee official said that when an institute prize is shared, "We are probably recognizing the recipients for equal efforts."

Questions of credit are sometimes so hard and so invidious to resolve that it seems fairer to blame the prize rather than the prize-giver for any inequity of distribution. "It's precisely questions of this kind that make one skeptical about the whole business of the Nobel prize," says George B. Field, professor of astronomy at Harvard. Field, whose attitude to scientific Brownie points is as detached as anyone's--he declined to be elected to the National Academy of Sciences--believes that science is really a social activity, not the endeavor of lone acting heroes which the institution of Nobel prizes presupposes. Be this as it may, the history of the discovery of pulsars admits of almost all the logically possible arguments on the award of the Nobel prize--that Hewish alone should have received it, that Hewish and Burnell should have received it together, that neither should have won it, and that Nobel prize money, founded at a time when science needed this kind of encouragement, should be put to less invidious and more truly useful purposes.

FLORENCE RENA SABIN

NOVEMBER 9, 1871 -- OCTOBER 3, 1953

by Philip D. McMaster and Michael Heidelberger

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On April 25, 1925, the National Academy of Sciences elected to its membership, for the first time in the sixty-eight years of its existence, a woman. She was Dr. Florence Rena Sabin, Professor of Histology in the Johns Hopkins Medical School, the first woman to be a full professor in that institution, and also the first woman to be President of the American Association of Anatomists.

It is to the credit of Florence Sabin that, by her life, her manner of working, and her scientific integrity, she was able to convince the university administrators of this country that a woman can be quite as capable as a man in the pursuit of research in the pure sciences. By her example she did more than any other person to open the careers of scientific investigation in laboratories, medical schools, and hospitals to women. Besides this, as we will endeavor to outline, she lived several unique lives, some concomitantly, some in sequence. Hers were not the lives of a Dr. Jekyll and Mr. Hyde, however, since she served Medicine, Science, Public Health, and even Politics, all to their great advantage.

Part of the significance of her life and her first two careers has been pointed out in Edna Yost's American Women of Science.

For the young woman of the modern day, one of the chief interests of her story lies in the fact that if Dr. Sabin had been able to read, at sixteen or twenty, this very story about what some American woman of science was to be able to accomplish by 1940, she would have had no idea that she possessed within her the capacities for the accomplishments recorded. The answer as to why she has been able to do what she has done lies, in-so-far as it lies in any single answer, in the quotation she has chosen for her bookplate--fifteen words credited to Leonardo da Vinci centuries ago--"Thou, O God, dost sell unto us all good things at the price of labour." (1)

On November 9, 1871, fifty-four years before her election to the Academy, she was born in Central City, Colorado, the daughter of George Kimball Sabin and Serena Miner Sabin.

Just before the Civil War, the discovery of gold was reported near her birthplace not far from Denver. The find brought many men to the region, indeed so many that a line of stagecoaches was established to

transport them. Among these men was George Kimball Sabin, born on a Vermont farm, the descendant of a Huguenot, William Sabin, who had come to Rehoboth, Massachusetts, in 1643. George Sabin's father had practiced medicine in the country, and the son seems to have leaned in the same direction, but in 1860 he gave up the idea of following the medical profession and left for Colorado to become a mining engineer. At this time Serena Miner was teaching school in the South, but during the Civil War she moved west to teach in a small school in the little town of Blackhawk, Colorado, where she met and married George Sabin in 1868. There were two children, Mary and Florence. In Central City this family lived in a small two-story house placed on the side of a steep hill, so that a stone retaining wall was needed to keep the ground in front from washing away. Their life must have been primitive, for water was peddled from door to door and stored in two large barrels. We are told by Florence's older sister, Mary, that a stairway led up from the dining room to a door which opened onto a bridge extending from the upper story to the side of the hill on which stood a barn for a cow and horses. No plumbing, no gas, and no electricity were in this house, which is still standing but now has these luxuries. When Florence was only four the family moved to Denver, to allow the older child to attend school, but even there, so this older child has reported, life was crude and both children could recall the fear with which they used to watch Indians marching single file down the street.

Three years later Florence's mother died, and for the next year both children went to a boarding school, Wolfe Hall. Still later their father took them to the home of his brother, Albert R. Sabin, in Lake Forest, Illinois, where they attended a private school for two years. In the summers they traveled, with an aunt, to their grandfather Sabin's farm near Saxtons River, Vermont. At the age of twelve Florence was left in the home of her grandparents, while her sister Mary returned to Illinois.

In January 1885, the family's eastern background asserted itself, and both sisters were placed in a boarding school, Vermont Academy, in Saxtons River, where the teaching was reputed to be unusually good. Both sisters received an excellent preparation for college, and four years later Florence entered Smith College, where her older sister had been for two years. There they lived together, not in a dormitory, but in a private house near the college.

Florence seems to have developed an early interest in science while she was still at school, since in college she took a course for the degree of Bachelor of Science, which she obtained in 1893. Mathematics and the laboratory attracted her most, and her work in both was of the highest order.

Little is known of the development of her talents, but it is certain that her life in college was free from hardships and stresses so often found in the lives of many famous persons. She tutored other students in mathematics, but this did not constitute an occupation arising from need. It has been pointed out by Edna Yost that Florence Sabin's life should offer comfort to those who inwardly feel no certainty of high intellectual

gifts but only a will to work, for in high school and early in college she seems to have had no inkling that she was to become a medical research worker or a scientist, still less one of the great woman scientists. Instead it would seem that her well-educated parents felt that she should have as good an education in the East as could be afforded. Consequently there was no struggle for an education. However, as has happened to many who have subsequently found themselves in experimental medicine, a course in zoology afforded the spark that kindled her interest in biological science.

It seems quite certain that at this time, if not earlier, she developed one of her best-known traits, a burning desire to strive for equal rights for women and to demonstrate that trained women could be the equals of the best-trained men. This attitude, of which more will be said later, may well have decided her choice of medicine as a career. A significant event, however, undoubtedly played an important part. While Florence Sabin was still in college the plans for a Johns Hopkins University School of Medicine, a school to excel all others in this country or abroad, had been delayed for want of sufficient funds. In the year 1890 a group of ladies headed by Miss M. Carey Thomas, later to become the President of Bryn Mawr College, and Miss Mary E. Garrett, both of them daughters of Trustees of the University, founded "The Women's Fund Committee" to gather money for the project, provided that women should be admitted to the school as well as men. By 1893, the year of Florence's graduation from Smith College, half a million dollars had been raised, most of it furnished by Miss Garrett, and the Johns Hopkins Medical School was open to both women and men. We do not know, but it seems likely that the possibility of entering this medical school of the highest standards, on an equal footing with men, may have influenced Florence--already engrossed in zoology--to decide upon medicine as her career. Perhaps, and again we do not know, this happening was purely fortuitous.

We do know, however, that lacking the necessary funds for four years of medical education, she accepted an offer from her old school, Wolfe Hall in Denver, to teach mathematics. Two years later she returned to the East and served for a year as an Assistant in Zoology in the Biology Department at Smith College, and that summer, 1896, she worked in the Marine Biological Laboratories in Woods Hole.

At the end of the nineteenth century women were not accepted in the medical schools of the great universities. Although there were women's medical colleges, it was extremely difficult to obtain an internship after graduation. The opportunity for a woman to enter the Johns Hopkins Medical School overcame all these difficulties. Thus fortune played into Florence Sabin's hands, and in October 1896, she began her first year as a medical student.

Her years of teaching, her excellence in mathematics and science together with her high standing in college, qualified her for entrance, much to her heart's content--as an equal of the men.

The First Career: Medical Student and Investigator

The First Great Influence: Dr. Franklin P. Mall. In an effort to obtain an outstanding faculty, the founders of the Johns Hopkins Medical School selected, among others, a man who happened to exert the greatest and most lasting effect upon young Miss Sabin. He was Dr. Franklin Paine Mall, the school's first Professor of Anatomy.

In the curriculum of medical schools, as generally arranged, anatomy becomes the student's first course. Dr. Mall, then only thirty-one years of age, had graduated from the University of Michigan and had studied three years in Germany with the embryologist His and the physiologist Ludwig and in this country for some time at Clark University. He had then become Professor of Anatomy at Chicago before coming to Baltimore. It is necessary to say something of Mall's philosophy of teaching in order to understand Dr. Sabin's success because she was first influenced by Mall and, later, she carried on with his methods. While in Germany, Mall had become convinced that all students, especially students of anatomy, should be allowed to work out their own salvation by being treated as fully responsible, independent adults. Mall sought to inspire the exceptional student with the investigative spirit, and to accomplish this end he never directed his students, either in study or research, to such a degree as to deprive them of the excitement of original discovery. Such teaching requires men whose own research has shown them the pleasure of discovery and the incentive to further research that follows from it. Mall's philosophy found a ready adherent in Florence, already trained by her courses in zoology in college and by her three years of teaching before coming to the medical school.

One can hardly overestimate the great influence that Mall had upon her, and it can only be appreciated by reading the admirable biography of him that was written by her nearly forty years later and more than fifteen years after his death. In this book, entitled Franklin Paine Mall: The Story of a Mind, his aims, described by her, accord completely with those she developed and championed later in life. It becomes clear from this book that Mall was her model. For example, she wrote in her preface: "The writer worked for twenty years under Mall; four years as a student, one as a Fellow, and fifteen years on his staff. Her start in research as a medical student and her opportunity for a career in scientific medicine she owes wholly to him. The theory of education outlined in these pages was thus learned directly from Mall." To anticipate further for a brief moment, she contributed, at Mall's request long before his death, to the well-known Keibel and Mall's Manual of Human Embryology.

To return now to the youthful Florence Sabin. While still a student in Dr. Mall's department she began her first investigation into the structure and function of the medulla and the midbrain. In this work models were constructed from serial microscopic sections of these regions of the central nervous system of a newborn infant. Reproductions of these models were used for many years in teaching at several medical schools. Later, shortly after her graduation, she published her first work, An Atlas of the Medulla and Midbrain. This accurate work has stood the

test of time, and by many of her colleagues has been considered to be one of her best contributions.

In spite of much time in research she carried on her studies well, and as she neared graduation, it has been said by those who were younger students that she was pointed out to them as an extraordinary person. Nevertheless, she never seems to have asked--then or later as a faculty member--for any favors because of her sex. She always considered herself on an equal footing with the men.

Following her graduation she received one of the greatest of opportunities for a woman doctor at that time, an appointment as an intern in the Johns Hopkins Hospital. This she accepted, but during her internship she realized that research and teaching held a greater appeal than the practice of medicine. However, there were no staff positions at that time for women in the medical school. Here again, however, her interest in the progress of women and the excellence of her work had attracted the attention of the altruistic group of ladies in Baltimore who had made possible the opening of the medical school. With the consent and encouragement of Dr. Mall, they created a Fellowship in the Department of Anatomy for Dr. Sabin. Here she began, under the best of auspices, many years of work in a new field, the development and the structure of the lymphatic system. Almost at once one of her first papers obtained for her the Prize of the Naples Table Association, which maintained, in the Zoological Station at Naples, a position or "Table" for the promotion of scientific research by women. It was given "for the best scientific thesis, written by a woman embodying new observations and new conclusions based on independent laboratory research." Within four years after her return to Baltimore Dr. Sabin was given the rank of Assistant, Associate, and then of Associate Professor of Anatomy. During this period a series of published papers brought her to the attention of the medical scientists throughout the world and established her reputation as an investigator of the first rank. By 1917 she was appointed Professor of Histology, the first woman to obtain a full professorship in the Johns Hopkins Medical School.

Nevertheless, the way to success was not simple. Her work began at a time when the anatomy of the smallest lymphatic channels and their relationship to the tissue spaces was little understood. Late in the nineteenth century many investigators believed that the finest branches of the arterioles became so small that blood cells could not enter them although the liquid plasma could. These branches, and even smaller vessels, then called the vasa serosa, were supposed to transport the liquid plasma to the lymphatics. Others thought that hollow connective tissue cells served this function. Some held that the ends of lymphatics remained open to allow fluid from the tissue to pour into them after its escape from the blood. Only a few thought that the ends of the channels were closed. At this time, too, embryologists were in disagreement about the way in which the lymphatic system develops. Dr. Sabin was able to write: "When I began my work, it was the accepted theory that lymphatics arose from the tissue spaces and then grew toward the veins," although, she added, both Langer and Ranvier had expressed the view, many years

before, that the lymphatic plexus grows outward from the veins by means of extensions of endothelium. However, their work had been done with relatively large pig embryos (about 90 mm. in length) and their views were not generally accepted since, as Dr. Sabin and others pointed out, the lymphatic system in pig embryos of that size is so well developed that it might almost be considered as adult in form. Accordingly she set herself to throw light upon these questions by studying the development of the lymphatic channels in very small pig embryos. By refined and difficult techniques, she succeeded in injecting these vessels with colored or black materials in embryos as small as 23 mm. in total length. These preparations indicated to her that the lymphatics develop in a manner opposite to that generally supposed, much as Langer and also Ranvier had suggested. That is to say, they arise as buds from the veins and grow outward as continuous channels by a process of further "budding."

Most of the other workers in this field employed serial sections of the fixed and dehydrated tissues of embryos for the study of the development of the blood and lymphatic vessels, and from these sections they reconstructed models of the tissues. In these reconstructions, the lymphatics seemed to arise from isolated connective tissue spaces lined with endothelium. To clear up the opposing views, Dr. Sabin combined the injection method with the technique of reconstruction from serial sections. For example, after filling the right jugular lymph sac and the lymphatics leading to it with colored materials (either Prussian blue or India ink), the lymphatics, in an embryo pig 27 mm. in length, looked like continuous channels extending outward from the jugular sac. Next, reconstructions were made from serial sections of this injected plexus and also from similar sections of the same area of the other side of the embryo, a region which had not been previously injected.

The plexus that had been filled with colored material could be reconstructed almost wholly, and showed lymph channels connecting with each other. The reconstruction of the uninjected side showed what appeared to be unconnected lymphatic spaces. Apparently the tiny connections between the uninjected, and consequently empty, lymphatics of the second reconstruction had failed to show up in the serial sections, whereas, when these channels were previously filled with colored material, the connections were rendered visible. The evidence obtained from many similar preparations indicated that only by previous injection of the lymphatics could their development and growth be properly understood.

In short, by this and her other work, Florence Sabin demonstrated that the lymphatics arise from veins by sprouts of endothelium, and next that these sprouts or buds connect with each other as they grow outwards from the veins toward the periphery, so that the entire system is derived from already existing vessels. Further, she showed that the peripheral ends of the lymphatics are closed and that they neither open into the tissue spaces nor are derived from them. These findings were later confirmed by Dr. Eliot Clark, who was able to observe the development of lymphatics in the transparent tails of tadpoles.

The reports of Dr. Sabin met with opposition from those who held the view that tissue spaces developed into lymphatics. The controversy became sharp, but her view, owing to the excellence of her observations and to her firm counteropposition, prevailed. Through it all she showed herself as willing and able as any man to fight for a good cause, an ability that came to full employment forty years later in her third career as a worker for the reform of the public health laws of Colorado. As she put it forcibly in a paper in which she defended her work against a number of critics: "The facts are correct and have been verified, the reasoning was correct and has been justified."

Dr. Sabin next attempted--since she had shown that lymphatics arise from endothelium of veins--to study the origin of endothelium itself. Having completed this study, which is too specialized in nature to be reported upon in this short memoir, and having already demonstrated the development of lymphatic vessels from endothelial sprouts, she next investigated the growth of blood vessels and the origins of the blood cells stemming from endothelium. Her previous work had been carried out, as already mentioned, by injections of colored substances into the lymphatic channels and by the use of serial microscopic sections of dead tissues. For the next work she used living tissue and the early methods of tissue culture which had just been devised by Ross Harrison. She was able to watch cellular growth in the "hanging drop" preparations and to see under the microscope the development of the earliest blood cells in explanted bits of the blastoderm of the chick embryo. She reported that on the second day of incubation of such cultures only red cells could be seen coming from the endothelial walls of the blood vessels. By the third day white cells appeared arising partly from new cells that differentiated from mesenchyme without becoming part of the vessel's lining.

There followed one of her many trips to Europe, this time to Leipzig and other German laboratories. Upon her return, she devised various techniques for the further study of preparations of living cells and for methods of staining them, while alive, with certain innocuous dyes. These methods of "supravital" staining became a most useful research tool for many workers, and they were employed in laboratories throughout the world. With this technique certain types of living cells, that could not be distinguished in the living state from one another at the time, took up her dyes; and the characteristic differences in the intracellular distribution of the dyes enabled workers to identify many of the cell types and to study some of their functions under normal and diseased conditions. Among these cells there appeared certain mononuclear units termed "monocytes," of which more will be said below. Her experiments indicated that these cells were involved in tissue reactions against infectious agents, particularly the tubercle bacillus. Accordingly, Florence turned her attention to the cellular defenses of the body against disease, and for the prosecution of this work a grant was given her in 1924 by the National Tuberculosis Association.

By this time the work began to bring her honors. She was elected to the presidency of the American Association of Anatomists, a recognition which paved the way, in the following year, 1925, to membership in the

National Academy of Sciences, at that time the only woman so to be honored.

From this short sketch it can be seen that during the years at the medical school her research had moved from purely descriptive studies of structure and of morphology of the nervous system and of cell types to more complex investigations of the development and physiology of living tissues and the functions of living cells. Her work with living monocytes began to lead her further, to a wish to understand the role of these cells in the complex mechanisms of the total defense of the body against infections; a subject which carried her to a second career in pure research, unburdened by teaching, and, forty years later, to her third career as a reformer of the public health laws and a fighter against tuberculosis in Colorado.

But first a word about her as a teacher.

Her Life as a Teacher

In the meantime Dr. Sabin had become even better known as a teacher than as an investigator. At the outset of her teaching career, while still young and a Fellow in the Department of Anatomy, she exploited her advantageous opportunity, that of being one of the first instructors to meet the first-year students as soon as they entered into the study of medicine. She, who had already captured the spirit of adventurous research from Dr. Mall, stimulated in these young men and women an interest in basic research, and she applied Mall's methods so well that she was able to influence, and to choose, many of the exceptional students to engage in original investigation in her department, while they carried on their regular studies. The many sides of her character, especially her scientific integrity, must have appealed to these students. (2)

It has been said that in her earlier years at the medical school she was rather formal, the result of early shyness of which there was no remaining trace as she grew older. Each year her reputation as a teacher increased, and her talks to the first-year students came to resemble a postgraduate course, since they attracted many upper-classmen and faculty members. She was as completely absorbed in her work as any teacher or professor might be. This characteristic may be illustrated by a story that has been told of a seminar in which a student had presented a patient with the diagnosis of apraxia, because he put his food, about to be cooked, directly into the fire. Hearing this, Dr. Sabin questioned the diagnosis, saying that just that morning--while thinking about her work--she had broken an egg directly on the stove with no pan for it. Her former students also say that--although she exhibited when young certain traits that might be considered somewhat masculine; for instance, objecting, as she did, to having men hold doors open for her--she was not a domineering woman at all. She was fond of stimulating discussions upon almost any serious intellectual topic, but especially those concerning the advancement of equal rights for women. Of course she backed the suffragettes, but in this she was by no means a reformer. She simply stood, with a certain rockline firmness, for the independence of all persons and an equality of opportunity for women.

Because of her later scientific eminence and integrity, because of the fact that she later became so well known as an exponent of new methods of teaching, because, still later, she became such an excellent public health officer and a proponent of new public health laws, as will appear presently, her influence for the improvement of the lot of women was immense. She accomplished much more by the example of her work than she could have as a mere advocate of reform, however vocal.

Both as a young woman and as an older teacher and adviser she was capable of making strong friendships. Early in her career she became something of a daughter to Dr. Mall. As soon as her students came to know her they found a warm-hearted woman, intensely human, always interested in her friends and in any liberal movement aimed at the betterment of human existence. As she grew older her feminine characteristics became more apparent. Students who showed an interest in extramural or nonmedical problems were promptly invited to dine with her and with her other friends. At such times, as described by all who received invitations, she showed a phenomenal flair for social entertainment in her home, a love for and skill at the art of cookery, and a knack for the preparation of dinners which would be "difficult to surpass in any place in the world," but carried on at times with fun and a pleasant burlesque of the exactness of scientific techniques. Some of her students or coworkers have described themselves as squatting on the kitchen floor with a long-handled fork and stop watch turning steaks exactly every third minute, on the dot of the 180th second; of scalding the dishes afterwards and cleaning them as though they were to be used for the next experiment in the laboratory.

During this period of her career her combination of sociability, domesticity, and scientific ability enabled her to establish an intimacy with her coworkers seldom found in the usual teacher-student relationship, and this intimacy, in turn, created opportunities to inspire them with her enthusiasm for research. Most of her eagerness to foster the student-teacher relationship, and her enthusiasm for it, must have been quite natural to her, but undoubtedly her earlier experiences in Germany engendered both. There, in the late nineteenth century, a spirit of academic freedom flourished, and the great physiologist Ludwig, with whom Mall had studied, set an example of friendly generosity. Through Mall's influence, Ludwig's attitude found reflection in Florence's proverbial generosity to students and colleagues alike. Although she had to plan and actually carry out much of the work for the inexperienced first-year students, she left the fruits of the work for them. Many of her students have mentioned her inexhaustible energy that enabled her to do much of the dull routine work of the laboratory so that the younger men would have more time for research. When they offered to help with the routine she would regularly refuse, saying, "You are young, you need time for study and research."

Before long, her natural warmth and interest in other people and their ideas gave rise to an intimate relationship with the progressively changing groups of students at Baltimore who were introduced to research by her, and this relationship, once established, was maintained for life.

Yet, in spite of this there remained a reserved formality. First names were not used. Nevertheless, fun was not lacking; not only at her home where the many dinners were cooked, but at the laboratory. There is a tale, for example, of a Saturday afternoon when hundreds of cheap plates in which cross sections of brains had been stained were set up in the cellar and shot to pieces to the noisy joy of all.

It has already been said that she considered herself to be on an equal footing with the men of the faculty. Indeed, since the head of the anatomy department was shy and sensitive and she was forthright, staunch, and ready at all times to take action to assure the progress of whatever she felt to be right, it fell to her to act as the buffer in the inevitable rivalries that arose, on high levels between academic departments, or on lower levels in the clashes of personalities, even among janitors and cleaning women. To the latter she showed a friendly benevolence which resulted in her becoming not only their confidante but often their financial benefactor as well. She took over all local problems of the laboratory and solved them with dispatch, whether it became necessary for her to extort an apology to the wife of the janitor from one of her group who had created an undue litter on the laboratory floor, or, on the higher level, to settle an old feud between two departments of the school concerning which one should teach the anatomy of the brain, a subject beloved by both.

Her interest in active teaching was surpassed only by her concern for education in general and for medical education in particular, especially for women. She frequently voiced her opinions in talks or speeches made when receiving some prize or honorary degree. Some of these talks appeared as leading articles in the weekly journal Science. For example, when asked to address the graduating class of the Woman's Medical College at Philadelphia, she chose for her topic, "The Extension of the Full-Time Plan of Teaching to Clinical Medicine." The opportunity gave her the chance to affirm her profound faith in the fitness of women, not only as practitioners of medicine, but as scientific medical investigators, even if married. She pled their cause as one of the next steps in the feminist movement and urged educated married women to take their share in professional work. In this talk she stressed, too, her conviction that all who teach in a profession that is constantly changing should contribute to that change by becoming investigators. She urged a shift "in interest by the leaders of the medical profession from the application of knowledge to the cure of disease to the study of the problems of disease." In her view, research--by focusing the minds of the ablest men in the profession on the problems of advance in medicine in contrast to the practical application of knowledge--could lift teaching to a higher plane.

Five years later she elaborated her views upon the relationship of research and teaching in medical schools before the Committee on Research on Educational Institutions of the American Association for the Advancement of Science. She urged the professor-investigators, as also did many others, to perpetuate themselves by stimulating the brilliant students to become teachers and workers in research, thereby furnishing the medical schools with worthy future faculties which would carry their

share of the burdens involved in the progress of medicine. Noting that some believed that no student should engage in investigative work, while others felt that all should do so, she asked: How should one introduce a student to research? Clearly, she said, the exceptional student can do more than the average, for whom the curriculum is arranged. He can find time for research, which should be encouraged, but only if he shows a strong bent for it. Some insisted that the students' scanty knowledge of what had gone before would prohibit research, but Dr. Sabin insisted that this difficulty could easily be overcome if the instructor, instead of smothering the student with a mass of facts, simply indicated the direction in which the important facts seemed to lead, since original minds, adapted for experimentation, do not revel in the analyses and classification of masses of detail but in looking forward. Indeed she pointed out that many of the most successful investigators did not analyze the previous literature until their work was already well under way, and that, for the student, the beginning of a problem often served best to stimulate and give purpose to later reading.

On the other hand, she asked, why advocate research for all? Does this not confuse the purposes of a medical education with the requirements of work for a degree of Doctor of Philosophy? Only a certain percentage of medical students need more than the knowledge of the methods of science, not training in experimentation itself, and only the exceptional student can obtain both. The teacher, she concluded, should be given complete freedom to decide how to furnish a few professional research workers for the progress of medicine. The intellectual quality of the teacher would be the essential requirement for the presentation of medicine as a growing subject, and for the emphasis on new points of attack and on new ideas which would themselves aid the student toward research.

Florence practiced what she preached and accomplished the ends she described by "the charm of ideas." The minds of the students were left free to judge the evidence of their work for themselves. Joint endeavor was seldom attempted for fear of degrading the student to the rank of a mere technician and of allowing him to fail to formulate his own ideas and to make his own plans. She insisted that, given the right teachers, the problem of who shall or who shall not do research will solve itself provided the teacher is left free.

Her success as a teacher showed itself in the large number of her students who later came to the front in research. Her career as a teacher was well summarized many years later, in 1951, at the dedication of the new Florence R. Sabin Building for Research in Cellular Biology at the University of Colorado School of Medicine, in an address given by Dr. Grover F. Powers, then Professor of Pediatrics at Yale University. During this ceremony at which, on her eightieth birthday, Dr. Sabin sat on the platform, as the newspapers declared, "full of years and honors," Dr. Powers said, in part:

Dr. Sabin is a great teacher. The impact of her teaching upon pupils is to inculcate a love of scholarship, a high regard for learning, a fostering of the spirit of

inquiry and of intellectual curiosity--for Dr. Sabin the ideal of great teaching is stimulation of the student to the love and pursuit of knowledge--kindling of the mind--upsurging of the spirit! It occurred to few students entering medical school shortly after the turn of the century (and before) that a great teacher was other than one who presented information and facts in an interesting and entertaining manner. The first vigorous and disquieting impact of Dr. Sabin upon pupils was to dispel that concept; soon, however, as a result of her teaching and example it was found by many that minds kindled and intellects awakened were more to be desired than minds well stocked with information, interesting and useful though it might be.

In her own teaching career Dr. Sabin perfectly exemplified the characterization she herself gave of one of her own teachers and colleagues. Using her own tribute to Franklin P. Mall, we say of Dr. Sabin, "Her contempt for slovenly or dishonest work, her admiration of rigidly perfected technique, her encouragement of objectivity in study, her insistence upon familiarity with the bibliographic sources, her emphasis upon the duty and pleasure of extending rather than merely acquiring knowledge, her impatience with inaccuracy and with stupidity, her unswerving loyalty to the highest ideals of natural science--all were qualities that made her a working companion of inestimable value to the young men and women who entered the Johns Hopkins Medical School."

And thus, in very truth, is characterized the great teacher we salute today!

The Second Career: Full-Time Investigator

By 1925 Dr. Sabin had reached a position of eminent distinction. Her great counselor and teacher Dr. Mall had died. The position of Professor of Anatomy and Head of the Department had not been offered to her; instead it was occupied by one of her former students. By this time, too, her cellular studies had begun to point toward an increased understanding of the total bodily defenses against infectious diseases, since the monocytic cells themselves seemed to take an important part in the defense against the tubercle bacillus. Dr. Simon Flexner, the Director of the Rockefeller Institute for Medical Research, who had long been interested in the humoral rather than the cellular mechanisms of defense against infections, concluded that a combination of investigations of both mechanisms was desirable. Accordingly, he urged Dr. Sabin to accept a position as full member on the Scientific Staff of the Rockefeller Institute.

Although this meant a new life in New York City, away from her Baltimore associates and her many friends, she accepted, and on May 1, 1925, a Baltimore newspaper carried a front-page column announcing the

planned departure of Dr. Sabin, the following autumn, to the Rockefeller Institute, there to be the first woman to become a full member of that institution. She was then Professor of Histology at the Johns Hopkins Medical School and the first woman President of the American Association of Anatomists. She had just added another "first" to her list. She was about to enter her second career, that of a full-time investigator.

In New York she organized and directed a group of younger investigators, some of whom had been with her in Baltimore, toward the continuation of her studies on the role of the monocyte and other white blood cells in the defense of the body against infections, and toward a correlation of this work with already existing knowledge about the humoral defenses. This group's previous work had shown the cell known as the monocyte to be the precursor of the well-known giant cell typical of the body reaction to tuberculous infection in animals and man. While Florence was at the Rockefeller Institute her studies on tuberculosis went forward rapidly. She became a member of the Research Committee of the National Tuberculosis Association. This committee, instead of creating a research institute, wisely decided on a plan, almost unique in the procedures of medical research at that time, to integrate, under Dr. Sabin's direction, bacteriological, chemical, and biological studies on tuberculosis through cooperative investigations by workers in already existing laboratories. Florence and her co-workers became part of this inter-institutional program which included pharmaceutical companies, state and privately endowed universities, and research institutes both private and federal. In this way, with somewhat limited funds, one of the best examples of the now modern approach to a complex problem was begun, and nationwide studies on tuberculosis were furthered. Throughout her years at the Rockefeller Institute she contributed, through this committee as well as by her fundamental researches, to a better understanding of the disease.

In short, after describing the well-known hard and soft tubercles that are produced respectively by virulent or avirulent tubercle bacilli, she and her coworkers showed that many of the lesions characteristic of tuberculosis could be reproduced in animals by chemical fractions extracted from the organisms. For example, tests with lipid fractions isolated from tubercle bacilli by Dr. Rudolph Anderson, of Yale, showed that certain fatty and waxlike substances could be identified within the phagocytic cells, a finding that made it possible to detect with assurance the cells reacting to these substances and to observe the subsequent behavior of these cells as they dealt with the materials.

Each of the fractions tested induced vigorous cellular reactions. The lipids, especially a phosphatide, gave reactions resulting in tuberclelike groups of epithelial or giant cells. Waxes stimulated the proliferation of fibrous tissue. A fat, soluble in acetone, caused hemorrhage in tissues and stimulated an overgrowth of connective tissue cells and blood vessels. A polysaccharide fraction appeared, at first, to attract leucocytes but then to kill them. The proteins of the tubercle bacillus seemed to be responsible for fever and an increase and localization of plasma cells. Clearly the tissue changes were induced not by the bacteria themselves but by various chemical substances in them.

Among these reactions Dr. Sabin considered the response of the monocytes most significant, and although the view was at first opposed and even ridiculed by already established authorities, the clarity and weight of the evidence she brought to light eventually tipped the scales in favor of her hypothesis. In one of her papers she wrote that "cellular and immunological reactions in tuberculosis center around the functions of the monocyte." She found that the phosphatide of the tubercle bacillus, and also one of its fatty acid components, induced the maturation of monocytes from more primitive cell forms. The phosphatide was "phagocytized by monocytes, which in dealing with it, become epithelioid cells which are usually arranged in tubercles similar to the hard tubercles of the actual disease." These epithelial cells often developed into giant cells of the Langhans type, cells that are characteristic of the lesions of true tuberculosis. She found "no evidence that the epithelioid cells dealing with the phosphatide return to the state of the original monocyte; rather, . . . after a long interval they died either singly or en masse, simulating caseation," another of the phenomena of the natural disease.

Besides the effect just mentioned, the proteins of the tubercle bacillus were shown by her group and by others to be the sensitizing substances, and, when combined with the fatty materials, to yield the counterpart of the allergy which was supposed, at that time, to depend on the infection itself.

These studies, carried on, of course, by many workers besides her own group, stimulated Florence's interest in the identification of the cells responsible for the formation of antibodies to infectious organisms. Her work with the monocytes had shown how these cells took up the antigenic substances. She turned next to a study of antibody formation, her last piece of basic research.

For many years before she began this work other observers had found that invading bacteria, carbon particles, and certain dyes are taken up by the lining cells of the blood vessels and by phagocytes of the reticulo-endothelial system, found chiefly in the lymph nodes, spleens, and livers of most animals. It was believed that these cells which took up foreign substances (antigens) formed antibodies. At this time a dark-red, coupled dye protein had just been produced by Heidelberger and Kendall, who had found it to be antigenic in rabbits. It was believed that this antigenic dye, if it could be seen inside cells after its injection into animals, would serve as a tracer to indicate something of the fate of antigens in general in the body and the way in which they might stimulate antibody formation.

Because of her long experience with cellular observations and the tissue reactions following the injections of foreign substances from the tubercle bacillus, Dr. Sabin was eager to make observations with this tracer antigen. An alum precipitate of this protein, after injection into animals, appeared in the reticulo-endothelial cells of various organs and especially in the phagocytic macrophages. The particles of the antigen engulfed by the macrophages appeared in vacuoles within their cytoplasm. Next, the dye became separated from the protein and could no longer be

seen. After a few days these cells began to shed their cytoplasm, and at the same time antibodies appeared in the blood. In every experiment the scavenger white cells of the blood took up the antigen, and many of these were in turn engulfed by the macrophages, as if the former cells played some role in bringing the antigen to the latter. Dr. Sabin concluded that the antigen was first rendered soluble in the cytoplasm of the macrophage where, in a second stage, antibody globulin was formed; later the antibody globulin and probably the normal globulin, too, were freed from the cell in the shed cytoplasm.

These excellent observations stand today, but, owing to their circumstantial nature, interpretation of them varies. Dr. Sabin appreciated that the shedding of the cytoplasm of these cells and the appearance of antibody in the blood merely coincided in time and might not be otherwise related. Under these circumstances the uptake of the antigen that she had observed might be only a first step, a preparation of the antigen for other cells that formed the finished product. Today this interpretation is the one that is most usually put upon the phenomena she described.

During this period as a full-time investigator Dr. Sabin had been associated for thirteen years with younger workers all of whom have since become well-known contributors to medical research. As one of them has put it, all were struck by her fund of energy, her enormous enthusiasm, and her great intellectual integrity.

The years in New York City were by no means devoted only to research. Life in New York City was truly a great adventure. Her leisure hours were filled with agreeable cultural diversions, and her interests were varied. She loved flowers and took much pleasure in the theater and in symphonic music, and thoroughly enjoyed many informal evenings of chamber music played in her home by some of the best-known musicians of the time. She read widely, mostly nonfiction and philosophy. Her breadth of interests increased all through her life. To illustrate, at the risk of anticipation: one of her coworkers has described a visit to her in Denver shortly before her death. She was found sitting before a television set--which was inactive--beside her on one hand a bowl of roses, on the other a volume of Freud, one of Einstein, Leo Stein's Journey into Self, Looney's Shakespeare, Identified, and Homer Smith's book on the development of man's religious ideas.

Nevertheless, in the laboratory in New York her energy for work never lagged. All through this part of her life, as later, she also exhibited the greatest enthusiasm not only for research but for public welfare; her eyes would shine and her face light up while discussing these subjects. Since she always advocated equality and equal opportunity for all, of either sex, scientifically, philosophically, or politically, she was therefore a true liberal. Another of her coworkers writes: "The great joy and pleasure which she derived from her work were like a contagion among those around her so that all were stimulated in much the same manner that she was. I have never known any other person to be quite so buoyed up by intellectual stimuli as she was. Dr. Sabin was one of those people who, however strenuous the previous day might have been,

waked at the crack of dawn with great enthusiasm for what the day was to hold. She was nearly always the first one at the laboratory, and greeted every one with a joie de vivre which started the day pleasantly for all of us. . . . Despite the fact that she was the first to arrive in the morning she was often the last to leave in the evening."

Although during the thirteen years in New York Dr. Sabin remained primarily a laboratory investigator, she maintained a continuous growth in outlook as well as in achievement. Early in life she had been an outspoken feminist with few doubts about the feasibility of business or professional careers for women. As she grew older she acquired a deeper understanding of the complexity of a career woman's life and of its difficulties, chiefly the conflicting claims of a home life with those of a professional career. As a result she came to view her own life as an exception. This must have been obvious to her, if for no other reason, because of the large number of honors, awards, and prizes which came to her from a variety of sources.

To illustrate the point it seems best to digress from the story of her career and to list most of these honors here, even those she received after retirement. Long before leaving Baltimore she had received early recognition from Smith College by the receipt of the Honorary Degree of Doctor of Science. Four years before leaving that city she was asked to speak as the representative of American women of science at a great reception for Mme. Curie held at Carnegie Hall in New York, and in her first year in New York she was elected, as already mentioned, to the National Academy of Sciences. A flood of honorary degrees came upon her: she received the Doctorate of Science fourteen times, first from Smith College, as just mentioned, and also from Mount Holyoke, Wilson, Oberlin, and Russell Sage colleges, the Colorado State College of Education, the Woman's Medical College of Pennsylvania, and the universities of Michigan, Colorado, Pennsylvania, and Denver, as well as New York University, Syracuse University, and Oglethorpe University. Goucher College gave her the degree of Doctor of Laws, making a total of fifteen honorary degrees. The Colorado State College of Education showed its further regard for her by naming one of its building Sabin Hall.

Her honors were not only academic; many came to her because of her work in behalf of the advancement of women's rights and, at the end of her life, because of her eminence in public health reform. For example, first, as already mentioned, while she was still an intern in the Johns Hopkins Hospital, came the Fellowship of the Baltimore Association for the Promotion of University Education of Women, with a stipend of \$800 for study in the Department of Anatomy with Dr. Mall. Next, after she constructed her models of the brain stem, she recieved the \$1,000 prize of the Naples Table Association for the best scientific thesis embodying new observations and new conclusions based on independent research by a women. In 1929 came the Pictorial Review's \$5,000 Annual Achievement Award. Two years later she was selected in a nationwide poll by Good Housekeeping magazine as one of the twelve most eminent women in this country. The following year, 1932, she received the National Achievement Award of the Chi Omega Sorority. Three years later (1935) her name

appeared on Mrs. Carrie Chapman Catt's famous annual list of the nation's ten outstanding women. In the same year, the M. Carey Thomas Prize in Science, with an award of \$5,000, was given to her at the ceremony for the fiftieth anniversary of the founding of Bryn Mawr College. More will be said of this presently. These diverse honors came to her before her retirement in 1939.

Thereafter she received, in 1945, the Trudeau Medal of the National Tuberculosis Association, given for "extensive studies of the physiologic activities of the chemical fractions of the tubercle bacillus," and two years later, at a fete at Rockford College, the first Jane Addams Medal for distinguished service by an American woman. Two weeks later, the National Conference of Christians and Jews offered its "Arts and Science" Citation jointly to Kate Smith, the singer, for the Arts and to Florence for Science, a partnership that gave her joyous amusement as well as sincere pleasure, and in the same year she received the Medal of Achievement from the University of Colorado. The following year, 1948, the American Women's Association presented her with its award for the year 1947, a medal for eminent achievement. In 1949 there followed the Elizabeth Blackwell Award at Hobart and William Smith colleges for her work in medical science, and, four years later, an Elizabeth Blackwell Citation, at the New York Infirmary, for achievement in medical education. In the meantime, in 1951, the Lasker Award of the American Public Health Association came to her for "outstanding achievement in the field of public health administration." The same year saw her presented with the General Rose Memorial Hospital's medal for distinguished public service, but, most important of all, in that year, too, the Florence R. Sabin Building for Research in Cellular Biology, part of the University of Colorado's Medical Center, was opened and dedicated to her. In the two following years the Sabin Award in Public Health was established in her name and she was given the Distinguished Service Award of the University of Colorado.

She was a member of many learned societies: the National Academy of Sciences; the American Association of Pathologists and Bacteriologists; the American Association of Physiologists; the American Association for the Advancement of Science; the National Tuberculosis Association; the Society for Experimental Biology and Medicine, of which she was a Counselor in 1932; the New York Academy of Medicine; the Harvey Society in New York; the New York Academy of Sciences, of which she became an Honorary Life Member; the Colorado State Medical Society; and the American Association of Anatomists, for which she served as Vice-President in 1908-1909 and as President in 1924-1926.

She belonged to the American Women's Association, the Colonial Dames, the American Association for University Women, and the Cosmopolitan Club, which extended to her after her retirement the rare privilege of living at its house, without dues, on all her annual visits to New York.

At various times in her careers Florence Sabin served on many useful committees and gave generously of her time and energy to their work. She served for years on the Editorial Board of the Anatomical Record.

She accepted the National Chairmanship of the Finney-Howell Foundation for Cancer Control, and served as Chairman of the Health Committee of the Colorado Post-War Planning Committee, as President of the Western branch of the American Public Health Association, as head of the Committee on Health and Charities of Denver, as a member of the Committee of Awards for the Guggenheim Fellowships, and also on the Committee of the National Society for Crippled Children and Adults. She was a director of the American Society for the Control of Cancer, and Honorary National Commander of the Women's Field Army. In her name there was created the American Association of University Women's Rocky Mountain Fellowship, the "Florence Rena Sabin Fellowship."

Most of these awards or prizes were clearly honors reserved for women; however, it seems worthwhile to stress again here the number of positions she obtained which were usually held by men, but which were occupied for the first time by a woman when she accepted them. She was the first woman to teach in the Johns Hopkins Medical School and the first to be appointed to a professorship. She was the first woman to become President of the American Association of Anatomists, the first to be made a Member of the Rockefeller Institute, the first to be elected to the National Academy of Sciences, the first woman Honorary Member of the New York Academy of Sciences, and the first recipient of the Jane Addams Medal. Other significant honors will be mentioned presently in considering Dr. Sabin's third career.

To return, now, from the preceding digression about her honors, to complete the story of her second career; she found time to give many lectures and talks, some to medical societies, others to various groups upon the receipt of some award. The enthusiastic and generous spirit shown in two of these talks, one presented before a medical group and the other to a mixed audience, was characteristic of her nature. In an address given in Denver, at the dedication of the Charles Dennison Memorial Library and the inauguration of the Henry Sewall Lectureship in Medicine, at the University of Colorado School of Medicine and its Hospitals, her own love of the adventure of research showed itself in almost every paragraph. From her ideas expressed in this talk it becomes clear, too, that her interest in the experimental pathology of tuberculosis had been aroused by Dr. Sewall, just as her early curiosity about the lymphatic system had been awakened by Dr. Mall.

In her response to President Park of Bryn Mawr, upon the receipt, in 1935, of the M. Carey Thomas Prize at the fiftieth anniversary of the founding of the College, she seized the opportunity again to carry on her campaign to further the acceptance of women in scientific posts. In her desire to aid the cause she--although receiving the prize for being the outstanding example of an American scientific woman--with characteristic modesty never mentioned her own work but only that of others. All through this talk she showed her gratitude to those who had recognized feminine ability, and she cited the importance of women in American medical science by stressing the role that M. Carey Thomas had played, as an earlier President of Bryn Mawr, in preparing women for scientific careers and the part she had also taken, still earlier, with Mary E.

Garrett in Baltimore, in obtaining the admission of women to the Johns Hopkins Medical School on an equal footing with men.

All was not work. Expressions of love and admiration came to her often. One among these was noteworthy. Only a few days before Dr. Sabin left the Rockefeller Institute to retire, her colleagues arranged a surprise dinner. Two of her associates paid a visit to her home and told her they had come to take her out to dinner at the famous Rainbow Room of the Rockefeller Center, a restaurant which she had never seen. She was not even suspicious until she was ushered into a room just below the Rainbow Room, filled with about forty of her former students and friends, all bound to her by ties of warmth and affection. There were six tables, each with an empty chair at its head, and Dr. Sabin took one course at each. During this dinner one of the eulogies described her as a hostess and provider of fabulous dinners, and her scientific career was briefly reviewed by the President of Johns Hopkins University, Isaiah Bowman, and by others.

Florence responded extemporaneously in a talk which illustrated, to all, her selflessness and her continued enthusiasm for the scientific career. After illustrating how her work was related to that of many others, she said: "The most interesting thing about it all is that, in the last few weeks, I have just discovered that everything I have been doing in these last few years is all wrong." This was said, of course, with tongue in cheek, but the remark illustrates her lifelong belief that both negative results and positive findings constitute the real progress of knowledge, not merely the startling discoveries of a few.

Several days later she left New York to take up residence in the West. Probably no investigator or teacher had ever left a more devoted group of students and colleagues. She carried with her the belief that satisfaction in life stems largely from work and activity. Accordingly it was foreordained that her retirement could not last. She was striding at the time, without knowing it, into a third career, and, although her entry into the new work was altogether unselfish, toward more honors and more esteem.

The Third Career: Public Health

Upon her retirement from the Rockefeller Institute for Medical Research in 1938 Florence Sabin went to Denver to live with her older sister, Mary, a retired teacher of mathematics in the Denver high schools, who was required to live in Denver by city statute in order to enjoy her pension. Happily this did not entirely sever Florence's ties with the East, for she not only continued an active correspondence with her numerous friends there but returned at least once a year during her service as a member of the Advisory Board of the John Simon Guggenheim Memorial Foundation (1939-1947) and the Advisory Committee of United China Relief (1941-1953).

In the meantime, the long connection of her family with Denver, her many friends of long standing in that city, and her immense reputation

speedily brought her into contact with research workers and their projects at the Medical School of the University of Colorado. She was also made an honorary member of the Denver Tuberculosis Society and was appointed to the Board of Directors of the Children's Hospital in 1942, on which she served as second vice-president in 1945 and 1947 and as third vice-president in

1946. Through these activities she became acutely aware of the archaic nature at that time of Colorado's laws relating to public health and the primitive methods for the enforcement of such laws as there were. With her still tremendous energy and clarity of vision she allied herself with a number of public-spirited citizens who had been fighting for better conditions under the leadership of Dr. Lloyd Florio but had won no support in the State Legislature as late as 1945.

In 1944 Florence was appointed to Governor John Vivian's Post-War Planning Committee and became chairman of a subcommittee on public health which soon was known as the "Sabin Committee" and was unique in its accomplishments. Its very active secretary was Herbert D. Moe, of the State Health Department. According to Mark Henry Harrington, at that time President of the Denver Tuberculosis Society and long an active worker for better health conditions in the city, Florence was an excellent chairman, presiding with vigor and dispatch, but always giving the other members full opportunity to speak their minds. Following a change in the State's administration, the work of this committee was actively supported by the new Governor, W. Lee Knous, who called Florence "the atom bomb." After intensive work, the committee, aided by Harrington, William McGlone, later Chairman of the State Board of Health, and Hubert Henry, all attorneys, drafted a legislative program, the "Sabin program," consisting of six bills. These measures (1) provided for a complete reorganization of the State Health Department, (2) allowed adjoining counties with limited resources to receive federal, state, or local funds in order to organize district health services, (3) set up a State Tuberculosis Hospital, (4) increased the per diem allowance for hospitalized indigent tuberculous patients, (5) established for the first time a strict control of brucellosis, then rampant among the cattle of Colorado and consequently a serious problem in public health, and (6) gave authority to the State Department of Public Health to receive federal funds under the Hill-Burton Act for hospital construction.

Then began the long fight to have these measures passed. In this there were some differences between members of the committee, as Florence did not support the bill for the state tuberculosis hospital, apparently believing that this could come later and that insistence upon it would weaken the chances of passage of the measures she considered more fundamental. However, she became the leading spirit in the struggle for passage of the others and, regardless of weather or season, went to every one of the sixty-three counties of the state campaigning for the bills. Illustrative of her activities during this period is the following item from the Colorado Springs Gazette-Telegraph of April 7, 1946: "Dr. Sabin . . . will speak on 'Public Health in Colorado' at a meeting at 8:00 P.M., Tuesday, at the Fine Arts Center . . . under the auspices of the Business and Professional Women's Club. . . . Other clubs invited to attend include

Altrusa, American Association of University Women, Soroptimist, League of Women Voters, Young Women's Christian Association, Business Women's Club, and the National Society of Secretaries." The article also included a short biographical sketch and portrait.

On one trip to address a mass meeting at Steamboat Springs her car was forced to turn back for tire chains by a blizzard. In spite of the cold and poor visibility she insisted on making the trip and actually arrived in time to make the principal speech. On such occasions, and there were many, when asked how she felt, she would always reply, "Fine as silk" or "Warm as toast," and she would telephone reassuringly to her sister as soon as possible. These far-flung local meetings were often followed by "post-mortem" stocktakings by the central committee, and Florence enjoyed these sessions in the smoke-filled room and had her drink with the men.

The picture of this stocky, indomitable, sincere, honest, knowledgeable, and famous elderly woman scientist campaigning vigorously for modern health laws caught the fancy of the public and served to dramatize the fight for their passage. In this the newspapers of Colorado played a large part, and many of them not only actively supported the campaign but delighted their readers as well with picturesque stories of its heroine and her energetic pursuit of every means of advancing the bills. In the state-wide elections of 1946, candidates for the legislature who had opposed her program or had not supported it were defeated in county after county, and even in the race for the governorship the winner, W.L. Knous, was the candidate who had most strongly advocated the proposed bills.

Many visits to state legislators then had to be made. Florence became a very effective lobbyist and spent much of her time in 1947 working directly with the lawmakers. Some of the politicians feared her because of her direct and energetic approach, but more and more became her loyal supporters and backed her strongly in the final sessions. Several of the key legislators were invited to attend the meetings of her executive committee. If any legislator asked her a question she was unable to answer, she immediately went to the State Department of Health and took back all information obtainable. As she herself said: "We got the facts and told people about them." In her work, Florence had the effective, active support of the Denver County Medical Society, the Colorado State Medical Society, and the Denver Tuberculosis Society.

The first tangible outcome of all this effort was the passage of four of the bills in 1947. The one for the establishment of a State Hospital for Tuberculosis failed to pass, as did also the bill for the control of brucellosis. However, Florence continued her fight for the latter, trying her best to work with the stubbornly resistant representatives of the cattle and dairy industries, and eventually, two years later, a compromise bill was passed in order to effect some improvement in the situation with regard to dairy cattle.

Now that the bills were passed, the principal task became one of implementation. This was never fully done because of the failure of the State Legislature to follow through with sufficiently large appropriations. However, a beginning could speedily be made, aided by a survey of health conditions in the State which had been carried out in 1945, under the direction of Dr. Carl E. Buck, then Field Director of the American Public Health Association, and financed by the Commonwealth Fund. Dr. Roy L. Cleer, who had for many years been Secretary to the State Board of Health and executive officer of its Division of Public Health, was appointed executive director of the Colorado State Department of Public Health, and, under the "county and district bill," set about encouraging counties to organize district health divisions. The first such unit comprised six counties in northeast Colorado, and a second district was created in the San Juan Basin in the southwest portion of the state.

After passage of the bills and the completion of a health survey of the city of Denver, sponsored by the Denver Tuberculosis Society, Florence was appointed Chairman of an Interim Board of Health and Hospitals of Denver by Mayor Newton in the autumn of 1947. As a public service, and at the suggestion of Dr. Florio, she later resigned from the Board and allowed herself to be named by the Mayor as Manager of the Denver Department of Health and Welfare, but donated her salary of \$4,000 to the University of Colorado Medical School for research. The Denver General Hospital, one of the principal activities of the Department, was, however, actually run by a director, Dr. J. P. Dixon, who was legally responsible to Florence, as was also the first Director of Health, Dr. Florio, during the period of his leave from the University. The interim arrangement lasted three and one-half years, until 1951, when a new City Department of Health and Hospitals and a Department of Welfare were legally created by an amendment to the city's charter, voted by its citizens. Dr. Sabin was, of course, named Chairman of the new Board of Health and Hospitals but resigned the office a year later, continuing only as a member under Harrington's chairmanship. An indication of her activities during this period is given by the following quotation from the March 1, 1951, issue of the Rocky Mountain News: "Dr. Florence Sabin yesterday began marshaling forces behind the proposed water pollution control act aimed at attacking the filth and stench of Colorado's sewage-laden streams." And again on November 3: "Dr. Sabin . . . was the force and spirit behind the Tri-County chest X-ray campaign a couple of years ago." This and other measures resulted in halving the death rate from tuberculosis in the city in two years.

On November 9, 1951, Florence celebrated her eightieth birthday, and on December 1 the School of Medicine of the University of Colorado observed the day by officially dedicating its new Florence R. Sabin Building for Research in Cellular Biology, in which her portrait adorns a wall, and by a birthday dinner at which she was presented with a bound volume of letters from her many friends, students, and associates.

Unfortunately, from this time on Florence was able to give less and less attention to public activities because of the time and care she wished to give her rapidly failing sister. The strain of this loving service was,

however, a severe one. In the opinion of a local medical friend, this could have been a direct cause of the cardiac decompensation, pneumonia, and other complications that resulted in a critical illness. While recovering slowly from this, Florence--always an ardent Dodger fan--settled down before her television set to watch the World Series game of October 3, 1953. She never knew the outcome, for as she sat there a heart attack ended her life.

When her will was opened it was found that this, too, was in character, for it provided that her estate, after her sister's death, be turned over to the Medical School of the University of Colorado.

Of the many newspaper articles and editorials that followed her death, two may be briefly quoted. The Denver Post termed her the "First Lady of American Science," and the Rocky Mountain News quoted Mayor Newton, in his expression of the city's grief, as saying, in part: "She was learned, she was wise, she was humble. She loved the world and every living creature in it."

As marks of esteem the new Sabin School, in Denver, has been named in honor of the two sisters, Florence and Mary. A bronze bas-relief of Florence has been placed in the Denver General Hospital, and she appears, among seventeen women, in a wooden panel, "Pioneers of Progress," carved by John Rood.

As is well known, each state in the Union may be represented in National Statuary Hall in the Capitol at Washington, by two statues. The first from the State of Colorado is a bronze statue of Dr. Florence Sabin, by Mrs. Joy Buba, of New York City. It was formally presented by the State of Colorado to the United States of America, on February 26, 1959. A replica will stand in Denver in the building of the Colorado State Health Department. Finally, the University of Colorado Press published in 1959 an excellent, comprehensive biography, Florence Sabin: Colorado Woman of the Century, by Elinor Bluemel. (3)

Footnotes

¹Quoted by the kind permission of the publishers, J.B. Lippincott Co.

²For the pictures of this part of Dr. Sabin's life, her "first career" as both a researcher and an investigator, the biographers lean heavily upon many pages of information generously given by some of her outstanding former students and other associates, now well known in their own fields. We are particularly grateful to Dr. Charles A. Doan, Dr. Lawrence Kubie, Dr. Kenneth Smithburn, and Dr. George W. Corner.

³In the compilation of this section, the writers wish to acknowledge with gratitude the cooperation of Dr. Roy L. Cleer, Dr. Lloyd Florio, Mr. Mark H. Harrington, Mr. Henry Swan, and Dr. Henry Swan, all of Denver,

Colorado; Mrs. Elinor Bluemel, of Englewood, Colorado; also the help of Miss I.T. Aulls, of the Denver Public Library, and Miss Norma Johannis, of Dr. Cleer's staff.

Chronology

- 1871 Born at Central City, Colorado, near Denver.
- 1875 Moved to Denver.
- 1878 Death of her mother.
- 1879 Attended a boarding school, Wolfe Hall, in Denver.
- 1880-84 Lived in the home of an uncle, Albert R. Sabin, at Lake Forest, Illinois; attended private schools and spent her summers in the East on her grandfather's farm at Saxtons River, Vermont.
- 1885 Entered the Vermont Academy at Saxtons River, Vermont, graduating in 1889.
- 1889 Entered Smith College, Northampton, Massachusetts. Chief interests: Zoology and Mathematics.
- 1893 Degree of B.S. from Smith College.
- 1893-95 Taught mathematics at Wolfe Hall, Denver.
- 1895-96 Substituted for an Instructor in Zoology in the Biology Department at Smith College.
- 1896 Spent the summer at the Marine Biological Laboratories, Woods Hole, Massachusetts.
Entered the Johns Hopkins University School of Medicine.
- 1897 Her first publication while still a student (see bibliography).
- 1900 Degree of M.D. from the Johns Hopkins University.
- 1900-01 Served as an intern in the Johns Hopkins Hospital.
- 1901 Awarded a Fellowship by the Baltimore Association for the Advancement of University Education for Women, to work in the Department of Anatomy of the Johns Hopkins School of Medicine, under the direction of Professor Franklin Paine Mall.
Published An Atlas of the Medulla and Midbrain.
- 1902 Assistant Instructor in the Department of Anatomy.
Published papers on the origin of the lymphatic system.
- 1903 Awarded the Prize of the Naples Table Association.
- 1903-05 Associate in Anatomy.
- 1905-17 Associate Professor of Anatomy. Further publications on the origin of the lymphatic system and of blood vessels.
- 1910 Honorary degree of Sc.D. from Smith College.
- 1917-25 Professor of Histology in the Department of Anatomy in the Johns Hopkins School of Medicine. The first woman to be elected to a full professorship in that institution.
Researches on the origin and functions of white blood cells and connective tissue cells.
- 1924-26 The first woman to become President of the American Association of Anatomists.
- 1925 Appointed a Member of the Rockefeller Institute for Medical Research: the first woman to be a full Member of that institution.

- Elected to the National Academy of Sciences: the first woman member.
- 1926 Degree of Sc.D. from the University of Michigan.
Member of the Research Committee of the National Tuberculosis Committee, coordinating the research of her group and others.
- 1926-38 Research on the pathology of tuberculosis.
- 1929 The Annual Achievement Award of Pictorial Review. Degree of Sc.D. from Mount Holyoke College.
- 1931 Degree of LL.D. from Goucher College. Named as one of America's twelve most eminent living women by a poll conducted by Good Housekeeping magazine.
- 1932 National achievement award of the Chi Omega Sorority.
- 1933 Degree of Sc.D. from New York University. Degree of Sc.D. from Wilson College.
- 1934 Publication of her biography of Dr. Mall, entitled Franklin Paine Mall: The Story of a Mind.
Degree of Sc.D. from Syracuse University.
- 1935 Degree of Sc.D. from Oglethorpe University. Degree of Sc.D. from the University of Colorado. Received the M. Carey Thomas Prize at the 50th Anniversary of Bryn Mawr College. Named on Mrs. Carrie Chapman Catt's annual list of the country's ten outstanding women.
- 1937 Degree of Sc.D. from the University of Pennsylvania. Degree of Sc.D. from Oberlin College.
- 1938 Degree of Sc.D. from Russell Sage College. Retired from the Rockefeller Institute.
- 1939 Degree of Sc.D. from the University of Denver.
- 1939-47 Served on the Advisory Board of the John Simon Guggenheim Memorial Foundation.
- 1942-46 Served on the Board of Directors of the Children's Hospital, Denver, and as vice-president for three years.
- 1944 Chairman of the "Sabin Committee" of the Governor of Colorado's Post-War Planning Committee.
- 1944-47 Actively campaigning for the passage of six public health bills proposed by her.
- 1945 Received the Trudeau Medal of the National Tuberculosis Association.
- 1947 Received the Jane Addams Medal for distinguished service by an American woman.
The Colorado Legislature passed four of the six bills for which she had been campaigning. These became popularly known as the "Sabin bills." Degree of Sc.D. from the Colorado State College of Education. Medal for Achievement from the University of Colorado.
- 1947-51 Appointed Chairman of an Interim Board of Health and Hospitals of Denver, and later, Manager of the Denver Department of Health and Welfare.
- 1948 The American Woman's Association Medal for eminent achievement in the year 1947.
President of the Western Branch of the American Public Health Association.

- 1950 Degree of Sc.D. from the Woman's Medical College of Pennsylvania.
- 1951 Chairman of the reorganized Board of Health and Hospitals in Denver. Received the Lasker award of the American Public Health Association, for outstanding achievements in the field of public health administration.
- December 1, at an official celebration of her 80th birthday, the University of Colorado dedicated the Florence Rena Sabin Building for Research in Cellular Biology.
- 1952 The Sabin Award in Public Health established.
- 1953 Distinguished Service Award from the University of Colorado. Died on October 3.

Posthumously

- A new school in Denver, the Sabin Public School, named for Florence Sabin and her sister Mary.
- A bronze bas-relief of Dr. Sabin placed in the Denver General Hospital.
- 1957 Dr. Sabin depicted among seventeen women, "Pioneers of Progress," in a wooden sculptured panel by John Rood.
- 1959 Statue of Florence Rena Sabin, by Mrs. Joy Buba of New York, placed in National Statuary Hall at Washington, and formally presented by the State of Colorado to the United States of America on February 26, 1959. Replica of the statue to be placed in the new Colorado State Health Department Building in Denver.
- A biography of Dr. Sabin, by Elinor Bluemel, published by the University of Colorado Press, Boulder, Colorado.

Key to Abbreviations

- Am. Assn. Adv. Sci. = Symposium Series of Publications of the American Association for the Advancement of Science
- Am. J. Anat. = American Journal of Anatomy
- Am. J. Med. Sci. = American Journal of Medical Sciences
- Am. J. Pub. Health = American Journal of Public Health
- Am. Rev. Tuberc. = American Review of Tuberculosis
- Anat. Anz. = Anatomischer Anzeiger
- Anat. Rec. = Anatomical Record
- Carnegie Inst. Contrib. Embryol. = Contributions to Embryology of the Carnegie Institution of Washington, Washington, D.C.
- Ergebn. Anat. u. Entwicklungsgesch. = Ergebnisse der Anatomie und Entwicklungsgeschichte
- J. Exp. Med. = Journal of Experimental Medicine
- Johns Hopkins Hosp. Bull. = Johns Hopkins Hospital Bulletin
- Johns Hopkins Hosp. Rep. = John Hopkins Hospital Report
- Nat. Acad. Sci., Biog. Mem. = National Academy of Sciences, Biographical Memoirs
- New York Med. J. = New York Medical Journal
- Physiol. Rev. = Physiological Review

- Proc. Assn. Am. Anat. = Preceedings of the Association of American Anatomists
Proc. Nat. Acad. Sci. = Proceedings of the National Academy of Sciences
Proc. Soc. Exp. Biol. and Med. = Proceedings of the Society for Experimental Biology and Medicine
Tr. ----- Annual Meeting of the Nat. Tuberc. Assn. = Transactions of the
----- Annual Meeting of the National Tuberculosis Association.
West. Med. Rev. = Western Medical Review
Woman's Med. J. = Woman's Medical Journal

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Science and Society

The results of scientists' activities extend beyond their individual spheres and affect our society and our culture. Resources are expended, and in return, ideas and technology are developed. Examples of the fruits of scientific efforts come readily to mind: computers, transistorized equipment, communication satellites, nuclear energy, devices that pollute and that control pollution, birth control pills, drugs and techniques to treat or prevent diseases, improved varieties of food crops, and so on. Because of these fruits, industries and governments have been willing to provide financial support for scientific research, but not without questions about what research to support and how much to pay.

In addition to goods and services provided to society, science contributes ideas that can influence the attitudes and values of the public. One of the ways scientific ideas come to be influential is by some scientists becoming influential people, for example advisors to the U.S. President. The other way is via ideas conveyed independently of individuals. Some scientific findings are so relevant to the societal conditions of the day that the ideas are used by influential non-scientists or in other ways become incorporated into prevailing public opinion.

In the United States, interactions between science and the government began shortly after the founding of the country. Early interactions involved the control of epidemics, exploration of the West, and early grants for technological developments, such as the telegraph. In the 1800s the government founded land-grant colleges to develop agriculture and the mechanic arts. Several federal agencies were established also, such as the Corps of Engineers, the Department of Interior, and the Geological Survey. World Wars I and II resulted in enlarged federal support for research and brought scientists into close interaction with the government. Currently the government supports both basic and applied research in numerous areas, particularly in relation to defense and health care. Debates about what research the government should support are closely tied to debates about national priorities. In making the necessary decisions, it is important that legislators and other citizens understand the scientific method and have realistic expectations of the potential contributions of science.

The following articles discuss some of these issues and questions such as: What influences have scientists had on U.S. politics? How has government support influenced the growth of science in the U.S.? What are the responsibilities of the gov-

ernment toward science? What responsibilities do scientists have toward politics and the public? Can and should research be controlled?

Science and Society: Two Views

The following article, a transcript of a speech given at Caltech as part of a lecture series, provides a recent (but instructively out-of-date) account of interactions between science and U.S. politics. Lee A. DuBridge received his Ph.D. in physics in 1926. During World War II he directed the Radiation Laboratory at the Massachusetts Institute of Technology in its work on radar. For 23 years he was president of the California Institute of Technology. He served as Science Advisor to then President Nixon from January 1969 to September 1970. At the time this article was written he was a member of the President's Science Advisory Committee and of the Science Advisory Committee of General Motors.

REFLECTIONS ON SCIENCE AND POLITICS

by Lee A. DuBridge

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I purposely entitled my remarks "Reflections on Science and Politics" for I don't have an analytical speech. I just want to talk about a few of the things that have occurred in my lifetime. In this period alone the relation between science and government, science and community, science and public affairs, science and practical affairs, has changed radically.

I was an undergraduate student in the years around 1920. At that time, anybody who used in the same sentence the words "science" and "politics" would have been thought to be slightly cracked. It would have been about as crazy as talking about walking on the moon. And, in 1920, it was. And about the same kind of thing happened. We did walk on the moon. Science did get involved in politics. Nevertheless, as one looks back, one can see developing traces of the relation between science and public affairs, science and government.

These traces became strongly exhibited during World War I. I found when I entered graduate school at the University of Wisconsin that several of the professors had been heavily involved in World War I through scientific research. The head of the National Research Council and one of the leaders of World War I scientific research was our own Robert A. Millikan. In World War I there was not very much time for U.S. science to be mobilized the way it was in World War II. Nevertheless there were, for the short period of time, quite substantial and some quite remarkable scientific

research and development projects in the fields of underwater sound, submarine detection, and of chemical warfare (responding to the first German use of chemical warfare in Europe). These activities initiated a lot of other things and immediately following World War I there was a very substantial increase in the tempo of development of American science. Before World War I American science was on a pretty small scale. There were a few brilliant investigators in various fields of science. Probably chemistry was the most active field in this country. Physics was just a struggling infant. But World War I made a great change. Many kinds of industrial and technological developments were initiated. I think you could say it created the chemical industry, which then really grew in this country after the collapse of the German chemical industry.

But I think most significant was the fact that the head of the wartime scientific effort, George Ellery Hale, then director of Mt. Wilson Observatory, and Robert A. Millikan concluded that it was time to extend this same kind of support of science into the world of civilian affairs--into peacetime. And so the National Research Council, created as a wartime mechanism for mobilizing science, became a peacetime supporter and promoter of science in the United States. One of the earliest and most valuable things it did, and Robert Millikan said he thought it was the greatest thing that happened to U.S. science in those days, was the creation of the National Research Council Fellowships. This plan allowed postdoctoral students from various parts of the country to be awarded fellowships on a merit basis and go to study wherever they chose. Caltech, even though it was a very youthful school in the 1920s, attracted a large number of these NRC Fellows. I was fortunate enough to be one of them during the years 1926-1928. I met many physicists here, both graduate students and postdoctoral fellows, who went on to very distinguished careers in science. That's when I first got acquainted with Carl Anderson, who for a few weeks as an undergraduate senior assisted me in my laboratory in the basement of the Norman Bridge Laboratory.

During the period 1920-1940 there was, relatively speaking, an enormous growth in the status of American science. We caught up, in a sense, to Germany, France, and England, which had been so far ahead of the United States that there was no comparison. By 1940 it was clear that science had developed in this country in a really spectacular way, starting from a pretty small base.

All of this happened almost entirely with support from nongovernment funds. The NRC was a private agency. Even though it advised and worked for the government, the only government funds it received were those to pay the expenses of its various investigations and advisory work. It raised the money for the NRC Fellowships entirely from private sources, primarily from the Rockefeller Foundation. As far as I can recall, the only government agency outside the field of agriculture that put money into university research was the National Advisory Committee for Aeronautics. Our own Guggenheim Laboratory here received from them what at the time seemed like a very substantial grant--I suppose \$20,000 for a year. But in the fields of basic science, physics, chemistry, mathematics, and biology, etc., government support was unknown during that decade.

It seems that it takes a disaster of some sort to change the rate of growth of science. World War II came along and the rate of growth of U.S. science was again sharply changed, to the vast improvement of its quality. During this war the Office of Scientific Research and Development was a civilian agency using government funds. It cooperated with the military, but was not under the military. It operated as a civilian establishment under civilian rules and regulations, receiving funds directly from the Congress as an independent agency. This was contrary to the World War I situation, when Dr. Millikan was not a civilian but a major in the Army Signal Corps. I won't go into the story of what happened during World War II; I think most of you are familiar with it. Let me now jump to the aftermath and what happened afterward.

Now it is clear on a nationwide basis, as it had not been after World War I, that science had something pretty important to do with the future welfare, prosperity, and security of the country. It would therefore be a good investment for the United States to make a strong effort, this time with substantial government funds, to promote the growth of science, to increase the number of scientists, to ensure the vast improvement of quality of scientific research, development, and teaching that was going on in the country. And so this happened. You all know the story of the expansion of the aircraft and electronics industries (stimulated by the work in electronics and radar and sonar during the war), of the development of computers, of the vast developments of nuclear energy for peacetime purposes as well as nuclear research, and of the beginnings of the work in rockets and later of missiles and space technology. There was tremendous acceleration of effort also in the fields of chemistry and chemical biology. The development and large-scale production of penicillin, for example, was an enormous stimulant to chemical, pharmaceutical, and biochemical industries in their research.

All these industries started broad, extensive, and largely government-supported research and development programs in their various fields, to the vast improvement of U.S. science and U.S. technology. And though we talk about university science as being terribly important to the country, we must remember that a large share of the funds for scientific research and development went to industrial and government laboratories during this period. True, the industries were interested in products, and their work was on the applied level. They were much more interested in those areas of basic science which seemed to have some relevance to their applied goals. But a lot of basic science, as well as an enormous amount of applied science and technology, was stimulated in the years after World War II through government funds.

At the request of President Roosevelt, Vannevar Bush developed, with a distinguished committee, a report on how the lessons of the fostering of research during World War II might be carried over into peacetime civilian research for the great civilian strength of the country. The result of this study was the famous report "Science: The Endless Frontier." Bush and his colleagues pointed out how critical it was to the future welfare, health, security, economic prosperity, and general welfare of all people to lay a better base of science and to pursue more energetically the knowledge of

nature, so there would be more science eventually to apply to the specific needs of people.

There was created, first, the Atomic Energy Commission to capitalize on the nuclear science and engineering developments during the war. The specific authorization and directive was given to the Atomic Energy Commission to develop basic research in nuclear science and related fields. Substantial programs of university research were thus supported during subsequent years by the AEC. In addition, in their applied work, support was given in both the military and the power fields.

To the Office of Naval Research I give credit for having seen the importance for a military agency to support basic science at the universities of the country. An extensive program was started by the Office of Naval Research to support research, primarily in the universities, in astronomy, nuclear physics, solid-state physics, in many fields of basic chemistry, biology, etc., to strengthen the basic science in the country. Later came the National Science Foundation, which carried on basic research as its sole mission. Then came the National Institutes of Health, with rapidly growing support in the years following World War II, and, finally, NASA. I mention these things just to give you a preview of the enormous developments that took place during the period after 1946.

I'd like now to talk about the relation of the scientists to the political activities and problems of the country as they have developed during the past 25 years. I mentioned the creation of the AEC. I guess this is probably the first time scientists got heavily involved in political arguments. The first bill that was introduced in the Congress to create an atomic energy commission frightened many scientists because they thought that it would lead to military control of atomic energy in the postwar days. They did not want this. I think those who worked on the Manhattan District during the war had had enough of military type of organization, and they wanted to be sure that the AEC was a civilian organization. Now I personally think that their fears about this first bill were misplaced. I was involved with Vannevar Bush, James B. Conant, Karl Compton, Robert Oppenheimer, and others in the drafting of that first bill and it was the furthest thing from our minds that anything like military control should be allowed, permitted, or should be fostered. Nevertheless, there was a little paragraph in the bill which provided that officers of the newly formed agency might be retired military officers or might be military officers who took leave of absence from military service. They would serve as civilians, but they could have been military officers. Well, this paragraph scared many scientists. The great battle that followed caused the form of the atomic energy agency that was to be set up to change radically and the present commission form established. I think that if we were doing it over again we would go back to the original bill. A commission of five people has been a pretty impossible administrative operation. Five bosses trying to run an enormous operation has proved politically extremely difficult, administratively almost impossible, and scientifically fraught with some serious and problematical defects. But be that as it may, this was the moment at which a large number of scientists in this country went to the congressional committees, wrote letters to the editor, and made speeches.

They declaimed against the potentiality of military control of atomic energy at the end of the war.

A second related effort of scientists in the political arena at that time was the development of proposals and agreements for the international control of atomic energy. There was a strong feeling, I think shared by nearly all scientists, as well as a large segment of the population, that somehow or other this enormous tool of human destruction should be internationalized. This would assure that no single nation or group of nations would have a monopoly of this terrible weapon. Well, those were very strenuous efforts, put out by many scientific people around the country--very conspicuously, by some distinguished Caltech people, Richard Tolman, Robert F. Bacher (who wasn't at Caltech then, but is now), Robert Oppenheimer, Charles Lauritsen. And though the United States failed in persuading the Russians to come into some kind of an international agreement, nevertheless the feeling for it was aroused, mechanisms for it were widely discussed, and active scientific participation in these discussions started then and of course continued actively and intensively down to the present day.

Now I think it's most fitting that Harold Brown is carrying on these same efforts and is leaving next week to take part again in the SALT talks, which is the present, 25-year-later stage of trying to get international agreements on the control of nuclear weapons. I think I've detected in the student newspaper here a couple of criticisms of Harold Brown--that he's been absent a bit. I think we at Caltech should be happy, proud, and glad that he's participating in these SALT talks. If there is to be any hope for an international agreement on the control of nuclear weapons, these SALT talks are absolutely critical. And it is absolutely critical that a man of his knowledge, capacity, good judgment, and devotion to the cause of peace should be the chief scientific advisor to our delegation at these SALT talks. I was delighted when he was named, and I hope that all of you will applaud him for the really self-sacrificing effort.

Well, this was a big political issue in the late 1940s. Another political issue that soon arose was the creation of what became the National Science Foundation. This had been the chief recommendation of the Bush report--that there be created a civilian agency for the sole purpose of supporting basic science in the country, primarily in the universities. Thus basic science would not be supported just as a secondary interest of the military, or as a secondary interest of the Atomic Energy Commission or any other mission agency. One agency would have as its primary function to support basic science and extension of human knowledge. Well, again there was a political battle as to what form this organization ought to take. As a matter of fact, it took several years for all the issues to be ironed out and for a bill finally to be signed by President Truman. He vetoed the first version because he did not think the administrative organization was right. I now think he was probably right, although at the time I didn't think so. Anyway, the National Science Foundation was created after, again, extensive political arguments. So, by 1950 it was clear that scientists were beginning to get a smell of the political world. They've had a lot since.

Actually, there wasn't much opposition to the idea of a science foundation--the argument was solely on the form it should take: the mechanisms and administrative arrangements. I must admit that one strong opponent to the NSF idea was Robert A. Millikan. He was deeply suspicious of the idea of government support of science in the universities and had deep fears of the political involvements that might ensue. I guess he was right! Yet we absolutely could not have had the science progress in this country of these recent years without the National Science Foundation. We've had to take the political problems along with the positive values that resulted from strong government support of science.

During all this time an extraordinary thing was taking place that we physicists and most chemists didn't pay much attention to, but a thing that was critical to the biologists and the people in medicine. That was the growth of the National Institutes of Health. Here was a real political story that I don't know whether anybody's written up or not, but it would be a fascinating reading. The National Institutes of Health had a strong leader in Dr. James Shannon. Dr. Shannon had a peculiar ability to get along with congressmen, at least with some congressmen, and he was able to find friends (Senator Lister Hill, I guess, was the chief one) in the Congress who were as strongly committed as he was to the improvement of the health of the American people through better research in biology and medicine and related areas. And so NIH was built up (many people say far too fast, but now I think we all rejoice that it is there) into a billion-dollar-a-year operation, supporting research at universities, within its own laboratories in Bethesda, Maryland, and other places around the country. The result has been a fantastic increase in our knowledge of the nature of life, the nature of biological systems--the physics, the chemistry, the biology of life--and of course this has spilled over in spectacular ways into the fields of medical science. Today there are many health areas in which we no longer have the same type of medical or hospital problems that we had 20 years ago.

It is sometimes said that our hospitals in this country today are of three kinds. The first are the birth-and-death hospitals, the ones that take care of the arrivals and the departures from this life. These will always be necessary. The second are the "body shops," for the repair of those who go skiing, get in automobile accidents, play basketball too hard, or whatever. We'll always have to have the body shops. The rest of the hospitals are "monuments to ignorance." They're there because we do not know how to prevent and cure the diseases they treat. We don't have any TB hospitals anymore, because we know how to prevent and cure TB. We don't have to have any polio hospitals anymore, because we know how to prevent polio. Just think how nice it would be if we didn't have to have any cancer hospitals, any heart hospitals, any cardiovascular hospitals. We now treat the symptoms and try to keep the patient comfortable. We can do so little about it because we don't understand the basic biology, chemistry, physics, and physiology of the disease itself. And this is why the continuation of the work of the National Institutes of Health is so important.

I think a spectacular example of the importance of NIH is the present situation with regard to cancer. A few years ago there was little hope that we would be able to do much in the field of cancer cures in the next few years. But there have been, through genetics, through biochemistry, through chemistry, and through many other fields, spectacular advances that give at least a cutting edge of an attack on the cancer problem. And that's why President Nixon recently proposed that an additional hundred million dollars in fiscal 1972 be devoted to trying to make more rapid progress in the field of cancer, to capitalize on the knowledge we've obtained from work in NIH and all the work conducted in universities around the country. I think this is a good political move on the part of the President, but I think it's a very solid and important scientific move and will have important repercussions in strengthening research in all the fields in biology and medicine related to the cancer problem. That's almost the whole of biology and medicine.

The growth of NIH was the result of a strong political alliance between Dr. Shannon, a powerful group of doctors and biologists, and a few senators and congressmen, all of whom were devoted to improving the health of the country. People had some suspicion that a lot of the congressmen who voted for the NIH thought they might die of cancer otherwise and had other, rather personal, reasons. But nevertheless they supplied the money, and while the National Science Foundation was struggling to get its money, the NIH was every year being given more than the president asked. In other words, NSF did not have the political punch behind it that the NIH had, and so NSF struggled along with its budget growing rather slowly. Not until after Sputnik did the NSF budget begin a really reasonably rapid rise.

Earlier, I said that it takes a disaster to stimulate progress. Sputnik was a disaster--at least to the pride and prestige of many American people. The Russians beat us to the first orbiting satellite. A wild wave of denunciation and a wild wave of determination to make things better swept the country in 1957. And again there was a rapid rise in the rate of growth of the support of science by the government. The Sputnik incident had many aftermaths for science, just as World War II [had].

And so if the 12 years between 1946 and 1958 were years of building a strong science foundation in the country, I can refer to the years between 1958 and 1967 as the Golden Age of Science--golden from the point of view of money. Sputnik stimulated large science appropriations and large requests by President Eisenhower and later other presidents. The Department of Defense, NASA, NIH benefited, as did the NSF and the AEC. All government agencies were given rapidly rising funds for basic research and for applied research in various critical areas.

One program was begun that would have an important impact. This was a much-enhanced and enlarged program of training of scientists and engineers. In 1946 there probably were not more than a dozen or 20 institutions in the United States that could be said to offer first-class Ph.D. work in the basic sciences. And the number who could be admitted to those institutions, while growing, was still inadequate for foreseeable

demands for scientists and for engineers. With the development of the NASA program it became clear that a large and increasing demand for scientists and engineers was in the offing. Large government funding was provided for fellowship and traineeship programs. Thus the number of graduate students working on research projects was greatly expanded during those years.

And then, too, an awareness developed that there ought to be many more first-class institutions giving good graduate work in the science and engineering fields. And so largely through the NSF, special funds were allocated to strengthen the second-level institutions--those below the first 20 but which had the possibility of improving their programs. And now, although the original 20 are still the top 20, there are certainly another 50 institutions which have vastly improved their graduate programs in the science and engineering fields in the last 10 or 15 years.

In 1958, President Eisenhower created the office of Science Advisor to the President and later the Office of Science and Technology to back him up. James R. Killian, Jr., was the first Science Advisor. He brought to the central part of government a topnotch group of scientists, some on a full-time basis, many others serving on a variety of advisory committees, to work directly with the President on the problems related to the fostering of American science and technology. This greatly strengthened the political position of science in the government. The Science Advisor was created as really a very high level position in the government--just below the cabinet level. There are only probably four or five out of the hundreds of members of the White House staff that have the same status, the same level in government, the same executive appointment that the Science Advisor has. So he became one of a pretty small group of senior advisors to the President of the United States. Different presidents use advisors in different ways, but the status is always there. And believe me, in Washington status symbols are terribly important. This meant that the Science Advisor was listened to in the halls of Congress. He was listened to in a variety of other political circles. Science finally found a voice that it had not had at all in the previous years.

And so, during the period from the end of World War II until 1967, the funding of university scientific research grew from essentially nothing to \$2 billion a year. For many years between about 1950 and 1965 the rate of growth was 21 percent per year, compounded. So you see, it grew like a bacterial colony. There were those who extrapolated that curve to see when it would exceed the gross national product. With the GNP growing at 4 percent per year, science funding growing at 21 percent per year, you can do a little logarithmic plotting and figure it out yourself. Clearly this couldn't go on forever.

Then in 1967 came the reaction. The end of the Golden Age. What happened? Well, the story is so complicated that it's impossible in the few minutes I want to take here to really recite it. I'm sure most of you know much of it anyway. Senator Hill died, Dr. Shannon retired, and the political support behind the National Institutes of Health leveled off sharply. The Apollo program reached the peak of its spending about 1967. NASA no

longer needed the huge \$6-billion-a-year appropriations, and so no matter what attitude the country would have had then, its appropriations would have certainly declined. But with the attitude of the country they declined even faster, from nearly \$6 billion a year to a little over \$3 billion now.

And then, of course, the horrible situation in Viet Nam grew, became of more concern throughout the country, throughout the government, throughout the Congress, and throughout the Administration. It had all kinds of reactions, some understandable, some a little strange. One, of course, was the reaction against the military support of science, even the military support of basic science in universities--the same kind of science that the Science Foundation was supporting. There was a reaction against the use of military funds for the support of any science. I think the reaction was an ill-conceived reaction, but it was there, and it certainly had a strong effect on the economics.

Then, too, there was the growing feeling of, "Well, yes, science has performed a lot of miracles, we go to the moon, we got the atomic bomb, but science isn't all that great. It hasn't cured cancer, it hasn't solved the problems of our environment, it hasn't solved the urban problem, the racial problem, it hasn't made everybody rich. Science isn't so good after all." Well, I believe these things were strongly expressed, in newspaper editorials (especially in the eastern press), in many letters to the editor, in many letters to congressmen, and in many speeches on the floor of Congress and on platforms around the country. People said, "We have overrated science all these years, it isn't so hot after all. Not only that but it's making things worse instead of better. Look at the smog, and look at DDT and look at eutrophication, and look at some other things." Well, this attitude, believe me, gets rapidly reflected in the halls of Congress and even in many parts of the Administration. There was a growing fear related to the distrust of technology and science: "What will they do next that will bring terrible tragedy and trouble to the people of the world?"

Another factor was certainly the troubles on the campus. This created very unhappy reactions in the halls of Congress and many parts of the Administration: "If the universities can't keep their house in order, why should the government give them any money?" And there were many moves to reduce the appropriations for any purpose to universities just because the students misbehaved and the administration wasn't able to keep them in line.

And so, for these and many other reasons, which coincidentally arose in the years 1967-1968, the academic research funding leveled off. It didn't really decline, taken as a whole, but since inflation was still going ahead, the net science that could be bought declined 10, 15, 20 percent maybe, over a period of three or four years.

Well, it was just at this unhappy period of American history of science that I took the journey to Washington. Just a little over two years ago now. And the first question was: "What's the Nixon administration going to do about this matter?" Well, the new President was faced with two serious problems. First, the previous administration had already prepared

the budget for fiscal 1970, which had to be submitted to Congress early in 1969. There was very little the new administration could do about any radical, sweeping changes in that budget. It would have taken six months of reworking to come up with a new one which was very much different. So only some tinkering with it was possible.

The first tinkering that the President felt had to be done was to bring down the deficit. The deficit had piled higher and higher during the previous years. Although President Johnson made strenuous efforts to reduce it in his last year and reduce it in the 1970 budget which he prepared, Nixon felt that still additional reductions were necessary. Hence a terrible budget crunch was the first thing that was faced. The budget crunch simply forced cuts in practically every agency and department of government, practically every activity except the uncontrollable ones. I discovered there are many expenses that even the President or the Congress can't control, for example, the interest on the national debt, social security, medicare, and many other expenses which have their own built-in formulas, which cannot be changed. Therefore, although Nixon did propose in fiscal 1970 a moderately good increase in the budget of the National Science Foundation, it was one of the very few agencies that got any proposed increase in the Administration's budget. Unfortunately Congress didn't see it that way, and instead of increasing the NSF budget as the President requested, by about \$40 million, they cut it by about \$60 million. The House tried to cut it by \$80 million, the Senate was a little more generous and only cut it by 40. They compromised at a \$60-million cut from the amount that had been requested.

This was bad enough, but it was compounded by the fact that a cut in the Department of Defense research budget was also made. Since the Department of Defense had been supporting substantial and excellent work in the universities, that was a real cut in available funding. And then to add to the problem, Senator Mansfield introduced his famous amendment saying that the Department of Defense could not spend money for research that wasn't directly connected with and pertinent to a military mission or purpose. This wiped out a lot of good scientific work that the Department of Defense had been supporting. With NSF's cut, it was not able to take over any of that work.

Now just as I was leaving Washington, the fiscal 1972 budget was being prepared and the picture there was substantially different, substantially better. The Administration decided last fall that in view of the economy of the country a balanced government budget was not possible. Indeed it was not desirable and therefore a deficit budget could be prepared again, as had not been allowed in fiscal 1970 and fiscal 1971. This meant, after the arguments had ceased (mostly after I left), that there was a \$100-million increase proposed for the National Science Foundation. It was really more than that because the NSF was relieved of some of its responsibilities, such as the Sea Grant Program, which made this an even larger actual increase. One hundred million dollars was also recommended for the cancer problems I've already mentioned. Many other R and D programs were increased. They are mostly small in size, but they may be very important for the future, such as new funding for environmental

research, for urban and transportation problems, and for urban housing and similar problems. Even though the research funds for some of these new agencies are small, a few million dollars, a 50 to 75 percent increase--100 percent in some cases--is about all they can take in one year. Management skills have to be built up in order to rapidly increase the ability to manage funds. But these are on the way up and a considerable boost is given in the proposed 1972 budget. I was glad to see also that with the re-creation of the Bureau of Budget into the Office of Management of Budget and the appointment of George Schultz as head of that, and Casper Weinberger, a wonderful Californian, as head of the budget part, a whole new attitude appeared in that office in regard to the support of scientific research in universities. So I have the feeling that we've turned the corner and that appropriations hopefully will follow these budget proposals.

But it isn't all that easy, maybe because Congress is still there. They can still cut the budget as much as they please, or increase it as much as they please. They still have the fears that they get from the rest of the country about the impacts of science and technology, about the problems on the campus, about whether we shouldn't be putting our money into other things. "Hasn't science gone too far, is it really all that valuable when we can think of other things that need to be done that science can't attack, etc., etc?" The best we can hope for, of course, is a return to a growth rate, not a decline rate, of the support of science. Certainly we expect a smaller growth rate than the 21 percent that we enjoyed during the Golden Age. How much of a growth rate? This, of course, remains to be seen. It will take maybe a 5 percent growth rate to match inflation; it will take maybe another 2 or 3 percent to match the actual rise in costs of scientific research as it gets more sophisticated, therefore more difficult and more expensive. But there is some hope. The basic science budget in 1972, as proposed by President Nixon, is about 13 percent above the 1971 budget, which is a fairly healthy rise and will be at least double the inflationary rise. There are also some changes in emphasis and content in the program. This is understandable and reasonable. Any program should be reviewed every few years and new directions sought that are pertinent to the times.

The saddest thing I think that is happening is that, because of the anticipated reduction of demand for scientists and engineers, particularly in industry, to some extent in universities, the programs for training more graduate students have been sharply tapered off. This has resulted largely from the actions of the Office of the Management of the Budget. It has analyzed, I think with a good deal of skill, the projected needs for Ph.D.'s in engineering and science in the next few years and it has come to the conclusion that the present rate of production and particularly the present rate of increase of production (we're producing 10 percent more scientists every year than we did the year before) is too large. At the same time the number of scientists being produced each year is 10 percent per year of the total number. Well, maybe this is wrong, but the question is: What is the proper level? The trouble is that nobody can really firm that up because we don't know what the demand will be in five, six, seven, or eight years from now when the present students are getting their

Ph.D.s, are completing their training, and are ready for jobs. I think it's risky to cut back too much on the training of graduate students. And yet it's happening. We only hope that somehow the slack can be taken up in future years. Perhaps universities can find ways to use research funds for training graduate students.

Well, a few words now about what I was supposed to talk about originally: What is the responsibility of the scientist and engineer, particularly the young ones who are coming along? There are going to be political battles on all these things that I've mentioned. What should we be doing about it?

Well, to the young scientist, let me say here, your first responsibility is very clear, and let me emphasize it with all my power. The first responsibility of a scientist is to be a good scientist. If you aren't that, then everything else is lost. If we don't have good scientists in this country, we might as well forget it. The reason science has contributed so greatly to the country, the reason it's grown so greatly and improved in quality, is because we have such a host of wonderful minds who have studied and worked in science during these many years.

And related to that is a second piece of advice: attack significant problems. Notice I did not say relevant problems. The word "relevance" is the enemy of lots of good basic science because you must ask yourself, "Will this cure cancer, will this cure our urban problems?" If you're working on problems in basic physics or chemistry or biology, you don't know, you haven't the faintest idea of what the knowledge gained is going to be in the first place, and what its application will possibly be in the second place. So relevance too insistently applied (as in the Mansfield amendment) is the enemy of good basic science, the search for knowledge.

It's not the enemy of applied science, because that's what applied science is. It's seeking to apply the knowledge you've gotten in basic science to particular problems important to our society, whatever they might be. Recall as much as you can while you're being a good scientist and working on significant problems (significant in the sense that they contribute to the advance of science)--recall and emphasize to your friends, to your students, the contributions of science to human welfare. We hear so much about the adverse effects of science that you might think that the people 200 years ago were so much better off than we are today, physically, mentally, spiritually. But think back to 200 years ago, when science had just started and compare the nature of an ordinary human being, the way he lived, the things he put up with, the kind of life he led, with what we have today. A large fraction of the population at that time, of course, were slaves or serfs. That's not true any more. And think of all the other ways in which knowledge, and the application of knowledge for the betterment of the human condition, has greatly advanced the human condition now, as compared to any time in human history. And understanding nature has been the essential element in the advance of human welfare during the last 200 years. As one understands nature, one can then open up new areas for applying the understanding to develop the mechanisms, the techniques, the technologies, that will do for us the things

that we want to do. The good things we want to do. And it's up to us to see that we use them for the good things that we want to do. But you can't do good things with science if there isn't the science available.

Finally, I think it's been evident from what I said that the public understanding of science, the understanding by newspaper editors, writers of letters to the editor, people in business, lawyers, congressmen, by politicians in all levels of all governments, is going to be essential if there's going to be a spirit which says that the extension of knowledge is important for the human condition, for the human race, and for the outlook of the human spirit. If we can promote that gospel, we may be sure that the reversal in the fortunes of science will be a real one in years to come.

(Discussion)

QUESTION: Could you tell us more about interaction between your office and Congress?

DUBRIDGE: Yes. That's a long, complicated story, I'm afraid. Of course, the Science Advisor reports to the President. As Science Advisor he has no connection with Congress at all. But, as it has been arranged, he also wears another hat. He heads a statutory office established by the Congress, the Office of Science and Technology. As director of this office he does report to Congress. He has to get from Congress a budget for the Office, for example, and he can be called to testify on matters pertinent to the activities of the Office. So it's a somewhat delicate position to be in. One must work closely with the President on matters of science in things that are coming ahead in the future. While the President is just evolving his ideas, his policy on them, one has to be in there early to give him the facts, give him the opinions of competent scientists and engineers on the scientific aspects of the issue. These things certainly cannot be made public, cannot be divulged to Congress until the President's policies have been formulated. And once he's made up his mind and announced his policy, then, of course, the Science Advisor supports that policy even though he may have had some reservations about it. As a member of the President's team, he stays a member of the President's team. That doesn't mean he has to lie about his own feelings, obviously. But he should, in general, either keep quiet or else support what the President has to say, even though the technical facts as he sees them may be against the decision. The President considers many others facts, economic, political, international, etc., which go far beyond the science and technology. Science and technology are usually a small part of the big decisions that the President himself has to make.

Nevertheless, we did have many Congressional contacts by every member of the staff (there are about 20 professional staff members in OST). Every member has a particular area of interest, a particular field of competence--geology, physics, biology, etc. He gets acquainted with the congressmen that are on the committees that are dealing with appropriations on legislation in that area. He prepares testimony, often testimony for the Director to use. And there is a great deal of interaction. But with a staff of 20 people, of course, the interaction isn't on that large a scale. It's

usually friendly, well informed, and fairly intimate, particularly with the staff of the congressional committees. These staff members are the most powerful people in Congress. As I found out, it's the staff of a committee that really guides the chairman as to the direction in which his committee is going to go. And they're very smart people, some of them, and working with them proved to be very fruitful. So there is a great deal of back and forth on an informal basis, but formally the Science Advisor advises the President, the cabinet members, and the White House staff people and does not attempt directly to advise Congress.

QUESTION: What about the members of the President's Science Advisory Committee, do you think that they should keep quiet about the way they feel?

DUBRIDGE: We had some arguments on that score and we never settled it. There were those on the Science Advisory Committee who felt, yes, as long as we were to be in the President's confidence, as long as he has referred confidential matters to us and listened to us in a confidential, private way, that we should not afterward, making use of the information and the ideas that we had got from that process, then go out and oppose the President's policy on scientific grounds. But many members didn't go along with the idea and some fellows followed their own.

QUESTION: Many of the schools that began expanding their graduate programs after World War II with government encouragement have had their funds dry up recently. Could you comment on their future? Do they contribute to diversification?

DUBRIDGE: Many new Centers of Excellence have been disappointed, and are going to be more so because the funds have been removed now and places that have just started to develop are going to be unable to continue. There've been some good results, some places have really come far along, often at the cost of the scientists they "swiped" from Caltech, MIT, Harvard, and the rest. Maybe they spread the talent a little bit but it's not too bad.

As to diversification, you know, there are millions of significant things in science to be done. There are so many problems, so many areas of ignorance, that there's no reason why two people have to do the same thing unless they do it for confirmation or together. But the styles run in a certain direction and you feel you're out of the significant areas of chemistry unless you're doing chemical dynamics, say. Diversification, of course, is absolutely essential. If you look back, look at the diversification there has been over these many years, the fields of science, the problems of science that were never thought of 20 years ago, that have grown. And that's the real problem for people like you in universities: to look at the significant areas in science, where we are lacking in knowledge, the things that would really help us to understand nature better and get those significant problems widely examined.

QUESTION: Do you feel that scientists have any obligation to control the use of science for evil purposes?

DUBRIDGE: Scientists control evil people? How can they do that? Scientists seek knowledge, and as they apply the knowledge, naturally they owe it to their conscience to apply it in the most useful and constructive way. But to prevent a particular piece of knowledge from being used by evil people? I do not see how a scientist can. He might declaim against it, but how he can stop it is something I'm unable to understand. Furthermore, for every piece of knowledge that's been used in a bad way, that same piece of knowledge, I bet you, has been used in a thousand good ways. I think you could give many examples. So, you discover nuclear energy--is that good or bad? Well, it was a great scientific and later a great technological achievement. My guess is that, in the long run, it will make life better for more people than it will make life worse for, provided first we can go ahead with this international control, for which I'm still very hopeful. But you don't even know what the results of an experiment are going to be when you start it. You can't possibly then tell how it's going to be used, if at all. Even when it's been out and confirmed and understood for many years, still someone can pop up with a new idea about how to use that kind of thing for something wrong--the problem is people then, not evil scientists.

QUESTION: There is quite an amount of disillusionment that I and other young scientists feel about the role of science in the community and I would appreciate your comments on the implications of this disillusionment for the future.

DUBRIDGE: Well, I don't know where to start. Certainly a lot of nonsense on this subject has been declaimed by important people (a lot of nonsense as I view it) but a lot of good sense also. I'm particularly worried about the nonsense in the environment field. When I went to Washington the first task President Nixon assigned me was to develop a mechanism for improving the government activities in environment protection. Suddenly, after not having had much to do with that subject in my life, I got interested and started looking into the problem to see what the government could do. We organized a cabinet committee and we began discussing what could be done in Agriculture, Interior, HUD, DOD, and NSF in the environmental field. Then the whole field blew up and everybody became an ecologist--some of them with the wildest ideas. They thought ecology was a new discovery, you know, that had just been made yesterday. They didn't realize that ecology had been studied by ecologists for many, many years. There were many things known about ecology that were now ignored, were not re-examined, not put in perspective. Is DDT a good or bad thing for the world? It saved half-a-billion human lives and destroyed quite a few pelicans and pigeons and salmon and other things. Now how do you balance the two? And this is a terrible thing that the people of this country have to face: Which do you prefer, the lives of half-a-billion human beings who would otherwise have died of malaria, yellow fever, and all the other insect- and lice-borne diseases, or the fact that certain kinds of fish and birds were adversely affected? Well, if you go from one thing to another throughout the whole field of ecology, you always find a question: What are the balances between the good and bad thing? Phosphates weren't put together with soap just for the hell of it or to make Lake Erie stink. They were put there by responsible scientists

and chemical engineers to convert soap into a better cleaning agent. And since we'd be completely out of tallow if we depended solely on soap now and didn't have detergents, it was a lifesaving thing that they invented detergent. And nobody's found a substitute for phosphate in detergent that's satisfactory. Various ones have been tried and seem to have worse environmental implications than phosphate itself. And phosphate is a small problem in the country as a whole. Even in Lake Erie it's only a small piece of the problem. But people say "abolish phosphate, abolish DDT," as though a sudden abolition of the thing that was developed and tried and worked over many years was either economically or healthwise possible or desirable. Well, the point is that one should look at all the substitutes for DDT that can be developed. There are many coming along, but, you know, some of them may be just as harmful as DDT when they're used on a large scale. It's going to take a long time to find out, and so I think we'd better go slowly in throwing away things that we know are going to be of enormous value to human beings before we throw in something of very doubtful future benefit.

Well, I don't know if this is relevant to what you say or not, but I think this confusion in the environmental field is something that the young scientists should carefully look at: What are the facts about these various things in which so much argument is being stirred up now in the environmental field? There are facts to be found out and I'm delighted that there is an environment laboratory starting at Caltech; they'll really find out about some of these things so that we can base our actions on knowledge and experience and not on emotion. And the same is true in so many other fields of the application of science to social problems--it's a terribly complicated field and we're not going to change social problems overnight by just a few scientists changing their line of attack: although many of them should, if they desire, and have the talents, move into the areas in which application can have a foreseeable social benefit. We've always had scientists, and we've had applied scientists and engineers, and we always, I hope, will. By and large, in the country as a whole, far more money and more people go into the applied areas than into the basic ones. It takes a special kind of quirk of the mind, a special kind of talent, to do significant basic science. And I would dread to think that many people with that peculiar and very valuable talent would not use it to its fullest. There are plenty of people who can do the applied work, many more than can do significant basic work in science itself. Don't you believe that, George? Not that it's any easier. You're shaking your head as though you didn't like what I said.

GEORGE HAMMOND: Applied science is hard!

DUBRIDGE: Yes, and so is basic work.

QUESTION: I feel the idea of a pecking order between applied and basic sciences should be done away with.

DUBRIDGE: I didn't intend to imply a pecking order, I just said the talents and interests were different. I don't think there's a pecking order between a physician and a physicist. They're just different kinds of

people with different kinds of talents and interest. I wouldn't hold one above the other, nor would I put the applied scientist above the basic scientist or vice versa. But I do think that without saying it's a better or superior kind of talent, that it is a different kind of talent that leads one to work imaginatively in the applied field as compared to what leads one to work imaginatively in the basic field.

QUESTION: Since the government encourages and supports graduate students, doesn't it have a responsibility to find jobs for them?

DUBRIDGE: No. I think the government has a responsibility for promoting science and engineering and the welfare of the country and in that process it will need a lot of scientists and engineers, at least it has in the past. But because it's contributed to the stipend of a graduate student I do not believe that the government is then responsible for getting that graduate student a job when he gets through. It's a private-enterprise business.

Now, we just had a talk with the President the other day (he met with the Science Advisory Committee) and he is terribly worried about the fact that too many scientists and engineers are not being fully employed or are not being employed at all. He's asked the Science Advisory Committee to study hard to see if they can come up with some ideas that will relieve the situation. He feels a responsibility. But I think really one should not say, "The Government has to get me a job because the Government paid for my education." You see, the whole rationale behind putting money into traineeships and fellowship on the part of NASA, and AEC, and DOD was: "We need ("we" = the agency) more scientists, we're not getting enough of them. The universities--Harvard, Caltech, etc.--are getting all of them and there aren't enough left over for us; therefore, let's train some more." So the whole rationale for increasing the funding of graduate study by stipends to students was the need of the agencies for more scientists. And then as NSF looked at the picture as a whole, it also saw the need, not for itself but for the country, of more scientists, and so it pushed ahead. Well, now when NASA didn't need any more scientists, they abolished their traineeship program. That's a natural reaction. If you were in charge of a budget that was limited and you were paying for the training of scientists and you didn't need them, why of course you'd quit. So did the DOD and AEC and so did the rest. NSF has struggled hard to keep its program alive. Its fellowship program is being maintained. But the administration, the Office of Management of the Budget, felt that the traineeship program should be cut back because, as I said, we were producing scientists at an increasing rate. Already it was hard to see how the production could be absorbed in the next ten years.

QUESTION: Isn't it irresponsible to train scientists you do not need?

DUBRIDGE: Well, I would say that, too. You can't make a day-by-day decision on this. You can tell how many automobiles to produce because you know how many sold last week. You know how many to produce next week. But with scientists, you start producing a scientist today, he becomes a scientist in six years, plus two more years as a postdoc. Eight

years have gone by and you have no idea what the demand is going to be then. So I think one shouldn't preguess the future too much. That's happened too often in the past.

Some pungent answers to questions about science and society are given by Max Delbrück in the following article, which was part of the same lecture series as the preceding article. Delbrück also illustrates his view of the main motivation for doing science. He began his career as a physicist and later switched to biological problems. In 1969 he received the Nobel prize in physiology and medicine for his studies on bacterial viruses.

We suggest that you compare this picture of science and scientists with your impressions of the people and events involved in elucidating the structure of DNA. Also compare it with the other biographies and abstractions about science found throughout this book.

HOMO SCIENTIFICUS ACCORDING TO BECKETT

by Max Delbrück

Reprinted from Science, Scientists, and Society, William Beranek, Jr., ed. Tarrytown-on-Hudson, N.Y.: Bogden and Quigley, 1972.

In December 1970 Bill Beranek wrote me a letter saying that he wanted one of these sessions devoted to the subject: "The Responsibility of the Scientist to Society with Respect to Pure Basic Research." He added a number of questions, which I will quickly answer, as best I can.

Question 1: Is pure science to be regarded as overall beneficial to society?

Answer: It depends much on what you consider benefits. If you look at health, long life, transportation, communication, education, you might be tempted to say yes. If you look at the enormous social-economic dislocations, at the prospect of an immense famine in India, brought on by the advances of public health science and nutrition science, at strains on our psyches due to the imbalance between technical developments and our limited ability to adjust to the pace of change, you might be tempted to say no. Clearly, the present state of the world--to which science has contributed much--leaves a great deal to be desired, and much to be feared. So I write down:

- (1) SCIENCE BENEFICIAL?
DOUBTFUL.
-

Question 2: Is pure science to be considered as something potentially harmful?

Answer: Most certainly! Every child knows that it is potentially exceedingly harmful. Our lecture series here on environmental problems concerns just a small aspect. The menace of blowing ourselves up by atom bombs, doing ourselves in by chemical or biological warfare, or by population explosion, is certainly with us. I consider the environment thing a trivial question, by comparison--like housekeeping. In any home, the dishes have to be washed, the floors swept, the beds made, and there must be rules as to who is allowed to produce how much stink and noise, and where in the house. When the garbage piles up, these questions become pressing. But they are momentary problems. Once the house is in order, you still want to live in it, not just sit around enjoying its orderliness. I would be sorry to see Caltech move heavily into this type of applied research. Again I write:

(2) SCIENCE POTENTIALLY HARMFUL?
DEFINITELY.

Question 3: Should a scientist consider possible ramifications of his research and their effects on society, or is this something not only difficult to do but perhaps better done by others?

Answer: I think it is impossible for anybody, scientist or not, to foresee the ramifications. We might say that that is a definition of basic science. Vide Einstein's discovery of 1905 of the equivalence of mass and energy and the development of atomic weaponry.

(3) CONSIDER RAMIFICATIONS?
IMPOSSIBLE.

So much for Bill's original questions in December.

I agreed to come to the lectures and then decide whether I thought I had something to contribute. After having listened to a series of lectures on environmental problems, such as lead poisoning, mercury poisoning, on smog, on waste disposal, on fuel additives, and to Dan Kelves' and George Hammond's more general talks, I told Bill that I had found the series interesting and worthwhile but that I felt most uneasy where I might fit in. So he wrote me another letter. Tenacious guy. With more questions. These again I can answer in short order.

Question 4: Why did you choose science as your life's work?

Answer: I think the most relevant answer that I can give to this question is this: I found out at an early age that science is a haven for the timid, the freaks, the misfits. That is more true perhaps for the past than now. If you were a student in Göttingen in the 1920s and went to the

seminar "Structure of Matter" which was under the joint auspices of David Hilbert and Max Born, you could well imagine that you were in a madhouse as you walked in. Every one of the persons there was obviously some kind of a severe case. The least you could do was put on some kind of a stutter. Robert Oppenheimer as a graduate student found it expedient to develop a very elegant kind of stutter, the "njum-njum-njum" technique. Thus, if you were an oddball you felt at home.

(4) WHY SCIENTIFIC CAREER?
A HAVEN FOR FREAKS.

Question 5: What is the history of your research?

Answer: Perhaps the most significant aspect is that it thrived under adversity. It thrived best under adversity. I have two periods in mind. The first was in Germany in the middle 1930s under the Nazis when things became quite unpleasant and official seminars became dull. Many people emigrated; others did not leave but were not permitted to go to official seminars. We had a little private club which I organized and which met about once a week, mostly at my mother's house. First just theoretical physicists (I was at the time a theoretical physicist), and then theoretical physicists and biologists. The discussions we had at that time have had a remarkable long-range effect, an effect that astonished us all. This was one adverse situation. Like the great Plague in Florence in 1348, which is the background setting for Boccaccio's *Decameron*. The second one was in this country in the 1940s during the war. I came over in 1937 and was in this country during the war as an enemy alien. And as an enemy alien I secured a job as an instructor of physics at Vanderbilt University in Nashville, Tennessee. You might think that this was a very unpropitious place to be, but it worked out fine. I spent 7½ years there. This situation gave me, in association with Luria (another enemy alien) and in close contact with Hershey (another misfit in society), sufficient leisure to do the first phase of phage research, which has become a cornerstone of molecular genetics. I would not want to generalize to the extent that adversity is the only road to effective innovative science, or art, but the progress of science is often spectacularly disorderly. James Joyce once commented that he survived by "cunning and exile" (and, you might add, by a genius for borrowing money from a number of ladies). I got along all right with the head of the Physics Department at Vanderbilt. He wanted me to do as much physics teaching as possible and as little biology research as possible. I had the opposite desires. We understood each other's attitudes and accommodated each other to a reasonable extent. So, things worked out quite well. At the end of the war I was the oldest instructor on the campus.

(5) HISTORY OF YOUR RESEARCH?
THROVE UNDER ADVERSITY.

Question 6: Why do you think society should pay for basic research?

Answer: Did I say that society should pay for basic research? I didn't. Society does so to a varying extent, and it always astonishes me that it does. It has been part of the current dogma that basic research is good for society, but I would be the last to be dogmatic about the number of dollars society should put up for this goodness. Since I answered the first question with "doubtful," I cannot very well be emphatic in answer to this one.

(6) SOCIETY PAY FOR RESEARCH?
HOW MUCH?

Question 7: How much control do you feel society should have on deciding which questions you should ask in your research?

Answer: Society can, and does, and must control research enormously, negatively and positively, by selectively cutting off or supplying funds. At present it cuts--not so selectively. That is all right with me, as far as my own research is concerned. I certainly do not think society owes me a living or support for my research. If it does not support my research, I can always do something else and not be worse off, perhaps better. However, the question, from society's point of view, is exceedingly complicated. I have no strong views on the matter.

(7) CONTROL OF RESEARCH BY SOCIETY?
A COMPLICATED MATTER, LARGELY OF PROCEDURE.

Question 8: Is there an unwritten scientific oath analogous to the Hippocratic oath which would ask all scientists to use their special expertise and way of thinking to guard against bad effects of science on society, especially today when science is acknowledged to play such a large part in the lives of individuals?

Answer: The original Hippocratic oath, of course, says that you should keep the patient alive under all circumstances. Also that you shouldn't be bribed, shouldn't give poisons, should honor your teachers, and things like that, but essentially to keep the patient alive. And that's a reasonably well defined goal since keeping the patient alive is biologically unambiguous. But to use science for the good of society is not so well defined, therefore I think such an oath could never be written. The only unwritten oath is, of course, that you should be reasonably honest. That is, in fact, carried out to the extent that, although many things you read in the journals are wrong, it is assumed that the author at least believed that he was right. So much so that if somebody deliberately sets out to cheat he can get away with it for years. The number of celebrated cases

of cheating or hoaxes would make a long story. But our whole scientific discourse is based on the premise that everybody is trying at least to tell the truth, within the limits of his personality; that can be some limit.

(8) HIPPOCRATIC OATH FOR SCIENTISTS?
IMPOSSIBLE TO BE UNAMBIGUOUS.

Question 9: Is science something we do mainly for its own sake, like art or music, or is it something we use as a tool for bettering our physical existence?

Answer: This is the question that turns me on. I think that it bristles with popular misconceptions about the nature of Homo scientificus, and therefore maybe I have something to say. Let me start by reading a few passages from an unpublished paper on this species, written in 1942 by a rather perceptive friend, a nonscientist:

The species Homo scientificus constitutes a branch of the family Homo modernibus, a species easy and interesting to observe but difficult and perplexing to understand. There are a number of varieties and subvarieties, ranging from the lowliest to the highest. We begin with the humble professorius scientificus, whose inclusion in this species is questionable, pass on up through the geologia and the large groups of the chemisto and biologia, with their many hybrids, to the higher orders of the physicistus and mathematicus, and finally to the lordly theoretica physicistus rarely seen in captivity.

Habitat: These animals range the North American and European continents, and are seldom seen in South America, Africa, or Asia, although a few isolated cases are known in Australia and Russia. (This was written in 1942.) Individuals of the lower orders thrive in most sections of Europe and America but those of the higher orders are to be found only in a few localities, where they live together in colonies. These colonies provide a valuable research field; here one can wander about noting the size, structure, and actions of these peculiar creatures. There is little to fear, for although they may approach one with great curiosity, and attempt to lead one to their lairs, they are not known to be dangerous.

Description: Recent studies of this as-yet-little-understood species have ascertained a number of characteristics by which they may be distinguished. The brain is large and often somewhat soft in spots. In some cases the head is covered with masses of thick, unkempt wool, in others it is utterly devoid of hair and shines like a doorknob. Sometimes there is hair on the face but it never

covers the nose. The body covering, when there is any, is without particular color or form, the general appearance is definitely shaggy. The male scientificus does not, like the cock or the lion or the bull, delight in flaunting elegantly before the female to catch her eye. Evidently the female is attracted by some other method. We are at a loss as to what this could be, although we have often observed the male scurrying after the female with a wuffley expression on his face. Sometimes he brings her a little gift, such as a bundle of bristles or a bright piece of cellophane, which she accepts tenderly and the trick is done. Occasionally an old king appears from the colony, surrounded by workers. He has soft grey hair on his face, and a pot belly. Scientificus is a voracious eater; this is not strange, for he consumes a great deal of energy each day in playing. In fact, he is one of the best playing animals known.

The scientificus undoubtedly have a language of their own. They take pleasure in jabbering to each other and often one will stand several hours before a group, holding forth in monologues; the listeners are for the most part quiet, and some may even be asleep. However meaningful this language may be to them, it is utterly incomprehensible to us. Perhaps the thing which endears this mysterious creature to us most is his disposition; although there exists a kind of slavery (the laboratorio assistantia being captured to do the dirty work), the scientificus does not prey on other animals of his species and he is neither cruel, sly, nor domineering. (The author had only studied the species for one year at that time.) He is an easygoing animal; he will not, for example, work hard to construct a good dwelling, but is content to live in a damp basement so long as he can spend most of the day sitting in the sun and rummaging among his strange possessions.

The paper then goes on into more detail about the biologia. We will let this suffice by way of a general description of Homo scientificus. The description is nice as far as it goes, but too superficial.

Now I want to switch gears and read another piece which I think goes to the heart of the matter. This is taken from the novel Molloy by Samuel Beckett. Beckett not only wrote plays--"Happy Days," "Krapp's Last Tape," "End Game," and "Waiting for Godot"--but also a number of novels that are less well known. This one, Molloy, published in the 1950s, concerns an exceedingly lonely and decrepit old man, and the whole book is a kind of a soliloquy that he writes down about his life. I have picked one episode that I hope will illustrate the point I want to make (without having to rub it in too much). . . . At the time of this episode Molloy is a beachcomber at some lonely place.

I took advantage of being at the seaside to lay in a store of sucking-stones. They were pebbles but I call them stones. Yes, on this occasion I laid in a considerable store. I distributed them equally among my four pockets, and sucked them turn and turn about. This raised a problem which I first solved in the following way. I had say sixteen stones, four in each of my four pockets, these being the two pockets of my trousers and the two pockets of my greatcoat. Taking a stone from the right pocket of my greatcoat, and putting it in my mouth, I replaced it in the right pocket of my greatcoat by a stone from the right pocket of my trousers, which I replaced by a stone from the left pocket of my trousers, which I replaced by the stone which was in my mouth, as soon as I had finished sucking it. Thus there were still four stones in each of my four pockets, but not quite the same stones. And when the desire to suck took hold of me again, I drew again on the right pocket of my greatcoat, certain of not taking the same stone as the last time. And while I sucked it I rearranged the other stones in the way I have just described. And so on. But this solution did not satisfy me fully. For it did not escape me that, by an extraordinary hazard, the four stones circulating thus might always be the same four. In which case, far from sucking the sixteen stones turn and turn about, I was really only sucking four, always the same, turn and turn about. But I shuffled them well in my pockets, before I began to suck, and again, while I sucked, before transferring them, in the hope of obtaining a more general circulation of the stones from pocket to pocket. But this was only a makeshift that could not long content a man like me. So I began to look for something else. And the first thing I hit upon was that I might do better to transfer the stones four by four, instead of one by one, that is to say, during the sucking, to take the three stones remaining in the right pocket of my greatcoat and replace them by the four in the right pocket of my trousers, and these by the four in the left pocket of my trousers, and these by the four in the left pocket of my greatcoat, and finally these by the three from the right pocket of my greatcoat, plus the one, as soon as I had finished sucking it, which was in my mouth. Yes, it seemed to me at first that by so doing I would arrive at a better result. But on further reflection I had to change my mind and confess that the circulation of the stones four by four come to exactly the same thing as their circulation one by one. For if I was certain of finding each time, in the right pocket of my greatcoat, four stones totally different from their immediate predecessors, the possibility nevertheless remained of my always chancing on the same stone, within each group of four, and consequently of my sucking, not the sixteen turn and turn about as I wished, but in fact four only, always the same, turn and turn about.

So I had to seek elsewhere than in the mode of circulation. For no matter how I caused the stones to circulate, I always ran the same risk. It was obvious that by increasing the number of my pockets I was bound to increase my chances of enjoying my stones in the way I planned, that is to say one after the other until their number was exhausted. . . . I should have needed sixteen pockets in order to be quite easy in my mind. And for a long time I could see no other conclusion than this, that short of having sixteen pockets, each with its stone, I could never reach the goal I had set myself, short of an extraordinary hazard. And if at a pinch I could double the number of my pockets, were it only by dividing each pocket in two, with the help of a few safety-pins, let us say, to quadruple them seemed to be more than I could manage. And I did not feel inclined to take all that trouble for a half-measure. For I was beginning to lose all sense of measure, after all this wrestling and wrangling, and to say, All or nothing. And if I was tempted for an instant to establish a more equitable proportion between my stones and my pockets, by reducing the former to the number of the latter, it was only for an instant. For it would have been an admission of defeat. And sitting on the shore, before the sea, the sixteen stones spread out before my eyes, I gazed at them in anger and perplexity. . . .

And while I gazed thus at my stones, revolving interminable martingales all equally defective, and crushing handfuls of sand, so that the sand ran through my fingers and fell back on the strand, yes, while thus I lulled my mind and part of my body, one day suddenly it dawned on the former, dimly, that I might perhaps achieve my purpose without increasing the number of my pockets, or reducing the number of my stones, but simply by sacrificing the principle of trim. The meaning of this illumination, which suddenly began to sing within me, like a verse of Isaiah, or of Jeremiah, I did not penetrate at once, and notably the word "trim," which I had never met with, in this sense, long remained obscure. Finally I seemed to grasp that this word "trim" could not here mean anything else, anything better, than the distribution of the sixteen stones in four groups of four, one group in each pocket, and that it was my refusal to consider any distribution other than this that had vitiated my calculations until then and rendered the problem literally insoluble. And it was on the basis of this interpretation, whether right or wrong, that I finally reached a solution, inelegant assuredly, but sound, sound. Now I am willing to believe, indeed I firmly believe, that other solutions to this problem might have been found, and indeed may still be found, no less sound, but much more elegant than the one I shall now describe, if I can. And I believe too that had I been a little more insistent, a little more resistant, I could have found them myself. But I was tired,

but I was tired, and I contented myself ingloriously with the first solution that was a solution to this problem. But not to go over the heartbreaking stages through which I passed before I came to it, here it is, in all its hideousness. All (all!) that was necessary was to put, for example, to begin with, six stones in the right pocket of my greatcoat, or supply-pocket, five in the right pocket of my trousers, and five in the left pocket of my trousers, that makes the lot, twice five, ten, plus six, sixteen, and none, for none remained, in the left pocket of my greatcoat, which for the time being remained empty, empty of stones that is, for its usual contents remained, as well as occasional objects. For where do you think I hid my vegetable knife, my silver, my horn, and the other things that I have not yet named, perhaps shall never name. Good. Now I can begin to suck. Watch me closely. I take a stone from the right pocket of my greatcoat, suck it, stop sucking it, put it in the left pocket of my greatcoat, the one empty (of stones). I take a second stone from the right pocket of my greatcoat, suck it, put it in the left pocket of my greatcoat. And so on until the right pocket of my greatcoat is empty (apart from its usual and casual contents) and the six stones I have just sucked, one after the other, are all in the left pocket of my greatcoat. Pausing then and concentrating, so as not to make a balls of it, I transfer to the right pocket of my greatcoat, in which there are no stones left, the five stones in the right pocket of my trousers, which I replace by the five stones in the left pocket of my trousers. At this stage then the left pocket of my greatcoat is again empty of stones, while the right pocket of my greatcoat is again supplied, and in the right way, that is to say with other stones than those I have just sucked. . . . Do I have to go on? No, for it is clear that after the next three series, of sucks and transfers, I shall be back where I started, that is to say with the first six stones back in the supply-pocket, the next five in the right pocket of my stinking old trousers, and finally the last five in the left pocket of same, and my sixteen stones will have been sucked once at least in impeccable succession, not one sucked twice, not one left unsucked. It is true that the next time I could scarcely hope to suck my stones in the same order as the first time and that the first, seventh, and twelfth for example, of the first cycle, might very well be the sixth, seventh, and sixteenth, respectively, of the second, if the worst came to the worst. But that was a drawback I could not avoid. And if in the cycles taken together utter confusion was bound to reign, at least within each cycle taken separately I could be easy in my mind, at least as easy as one can be, in a proceeding of this kind. . . . But however imperfect my own solution was, I was pleased at having found it all alone; yes, quite pleased. And if it was perhaps less

sound than I had thought in the first flush of discovery, its inelegance never diminished. And it was above all inelegant in this, to my mind, that the uneven distribution was painful to me, bodily. . . . I felt the weight of the stones dragging me now to one side, now to the other. So it was something more than a principle I abandoned, when I abandoned the equal distribution, it was a bodily need. But to suck the stones in the way I have described, not haphazard, but with method, was also, I think, a bodily need. Here then were two incompatible bodily needs, at loggerheads. Such things happen. But deep down I didn't give a tinker's curse about being off my balance, dragged to the right hand and the left backwards and forwards. And deep down it was all the same to me whether I sucked a different stone each time or always the same stone, until the end of time. For they all tasted exactly the same. And if I had collected sixteen, it was not in order to ballast myself in such and such a way, or to suck them turn about, but simply to have a little store, so as never to be without. But deep down I didn't give a fiddler's curse about being without, when they were all gone they would be all gone, I wouldn't be any worse off, or hardly any. And the solution to which I rallied in the end was to throw away all the stones but one, which I kept now in one pocket, now in another and which of course, I soon lost, or threw away, or gave away, or swallowed.

This is the parable of the Homo scientificus that I wanted to present. I want to stress two particular things in it. One is the uncanny description of scientific intuition. This is exactly the way Einstein must have struggled to explain the failure of all experiments attempting to demonstrate a motion of the earth relative to the "light-medium," until he very dimly realized that he had to abandon some "principle of time," the principle of absolute time, and this must have come in some such way as here described. Other people have described intuition in cases where they were able to reconstruct a little of it. Jacques Hadamard, the French mathematician, has written a little book, An Essay on the Psychology of Invention in the Mathematical Field, which is a collection of data on this phenomenon and describes how intuition wells up from completely unfathomable depths, first appears in a peculiar guise, and then suddenly breaks out with lightning clarity.

Second, let us look at Molloy's motivation. He certainly is not motivated by the goal of bettering our physical existence or desire for fame or acclaim. Does he do his work "for its own sake," "like art and music"? He describes in detail how his little game "for its own sake" becomes an obsession, beyond all measure of reason. This is not the way you and I do art or music, but it does resemble closely the way the creative artist and composers do it. You don't have to look at Beethoven to become convinced of that. Look at any child of five who is obsessed with a creative problem and breaks out in anger and frustration at his failures.

This obsessive fixation picks on anything, quite oblivious of its meaningful content of "revealing the truth about nature" or "bettering our physical existence." It is this quirk of our makeup, this sublimation of other psychic forces, that was delivered by evolution to cave man. More was here delivered by evolution than had been ordered. It carried us from cave man to space man, and may well carry us to our destruction. And why not? The little earthquake we had the other day should have served all of us as a timely reminder, if any reminding is needed, that we are not here to stay, not as individuals, nor as families, nor as nations, nor as the human race, nor as a planet with life on it. There is uncertainty merely as to the time scale.

The point I want to make is this. Man is not only Homo faber, the tool maker. The grand edifice of Science, built through the centuries by the efforts of many people in many nations, gives you the illusion of an immense cathedral, erected in an orderly fashion according to some master plan. However, there never was a master plan. The edifice is a result of channeling our intellectual obsessive forces into the joint program. In spite of this channeling, the progress of science at all times has been and still is immensely disorderly for the very reason that there can be no master plan.

So, what could we do if we decided that innovative science is too dangerous? I don't know, but one thing is certain: it would take a lot of manipulation of man--political, economic, nutritional, genetic--if you tried to control Homo scientificus.

(Discussion)

QUESTION: Returning to the third question, how can man with the characteristics you just mentioned resist considering implications of his work? This doesn't mean solving them--just considering them.

DELBRÜCK: I understood the question to mean: If I make a discovery, should I consider the implications and maybe not publish it even if it's a basic discovery? I think that it is impossible to foretell the implications. I couldn't agree more that you should consider the implications, say, of the genetic manipulation of mankind. You can't help it. It's of the utmost importance. Same with "zero population." I just don't consider this as the same thing as doing science, this business of considering the implications. . . . It's something entirely different, as I explained in answer to the second question.

QUESTION: It seems to me that many human beings are subject to neurotic obsession. But it's not clear how we, as scientists, choose problems. It seems to me conceivable that you might choose a problem because somebody tells you that it's an important problem for science. You can get upset about "why the hell can't I solve it" even if you don't care about the problem.

DELBRÜCK: I agree. Science gives the impression of being a magnificent cathedral, an enormous structure--a well-constructed thing, a cathedral

built by the continuous effort of many generations through many centuries. Of course, it isn't a cathedral because it wasn't planned. Nobody planned the scientific cathedral. To the student it looks as though it were planned. The student gets three volumes of Feynman lectures, 1300 pages of a splendid textbook titled Organic Chemistry, and other textbooks, and says "Aha! 150 years ago today they got this far. In the meantime all this was constructed, and now I continue here." My point is, that science is not that at all. Science is primarily playing willfully, and getting obsessed with it, and it is not being told: "Here, add your brick on page 1065 and do it properly or we won't give you a Ph.D." Such a student, if you ask him what he is doing, may possibly answer, "I am building a cathedral." More likely, he will say, "I am laying bricks," or even "I am making \$4.50 an hour."

QUESTION: Should we not think about how much society should support science?

DELBRÜCK: Oh, I don't want to think about it. It's a very complicated thing. Obviously the high-energy physicists want ever bigger machines that cost a hundred million, billion, etc., and they say the military spend more and the military say if we stop making war the economy will break down. These are all questions that are not very interesting. To me, anyway.

QUESTION: Would you be willing to relax a little bit on your attitude with respect to question 1, the question whether science is beneficial? Would you say this depends on how you define beneficial?

DELBRÜCK: Certainly. If we measure it in terms of energy production or infant mortality, then it's beneficial.

QUESTION: Well, I think it's very difficult to say what is beneficial.

DELBRÜCK: Yes. That's why I put a "doubtful" there. I didn't answer "no."

QUESTION: Most of the problem with science is that we don't even know what's beneficial to society.

DELBRÜCK: However, we can hardly evade the fact that the present state of the world leaves much to be desired, and that this is largely a result of the efforts of people like Molloy.

QUESTION: Then one might talk about whether the earlier stage of the world was an awful lot better.

DELBRÜCK: Of course you can. Please do. I don't feel like arguing.

QUESTION: Do you think it is common that scientists proceed in a way that is neurotic? Don't you think that occasionally they do something just because it's interesting?

DELBRÜCK: I didn't mean to use the term "neurotic" in a derogatory way. Our culture is a product of our neuroses--I mean a product of the diversion of psychic forces from their original function into other directions.

QUESTION: How could you do your research with such a pessimistic attitude? Did you have the same attitude when you started out?

DELBRÜCK: I can't answer that--how I was 40 years ago. If you call it pessimistic, I'm a very cheerful pessimist. I think there's something to be said for the pessimist. It merely means not glossing over some basic facts.

QUESTION: Your picture of a scientist is very personal, so your answer to the first question, "Is science beneficial?" should be "yes, it's beneficial to the doer." Molloy's pebbles were the same to him as special relativity was to Einstein and the hydrogen bomb to Edward Teller. The difference is that Molloy wasn't going to hurt anybody. Now, if you say that science is beneficial to the scientist because he gets satisfaction from it, and the scientist isn't thinking about the implications, does this imply that somebody else should think about the implications and say, "Molloy, you're OK; Einstein, you're doubtful; Teller, you're out"? Who shall make these decisions?

DELBRÜCK: My point was that that's quite impossible. Molloy and Einstein are identical. Einstein worried about relative motion experiments and came up with a solution of the paradoxes which implied, incidentally, an equivalence of mass and energy. This was just as esoteric as shuffling around the sucking stones. I mean that nothing could be more impersonal, impractical, more remote from any social implications than what Einstein did in 1905. To him, anyhow. Later, when the atomic arms race escalated one more round, and Einstein considered that he had been involved in their starting the atom bomb, both by inventing relativity in 1905 and by writing to Roosevelt in 1939, he regretted that he had ever entered science, but I don't think he really had thought about how deeply science is part of human nature. I think discoveries are all potentially equally harmful. Like the circulating of the sucking stones. Maybe Molloy is discovering a principle of permutation or number theory--God only knows the implications of this. Didn't the pictures look like some of the metal organic covalent bond shifting there? Didn't Harry Gray get an inspiration from it for something that's going to be utilized in some horrible contraption in a few years?

QUESTION: Can you draw a distinction in terms of creativity between Einstein thinking up ideas and Edward Teller making bombs--one being playful and the other being purposeful?

DELBRÜCK: I don't have to make this distinction because, if I want to control the bad effects of science, I have to stop Einstein. Why should I try to make a distinction between him and Teller? Teller is an excellent scientist. Although I don't know what he specifically did with the H-bomb, he certainly contributed a great deal to quantum mechanics and

chemical physics. So then the question is: Should the scientist stop publishing his science so that the bad appliers won't misuse it? Have a private club? I used to have a slide of a poster I found at MIT, a poster with a quotation from Einstein saying how sorry he was that he had ever, etc., and that if he could start life again he would just become a lighthouse keeper or something like that. Underneath on this poster there was an invitation from somebody saying: "Will you join us in a commune of scientists who will talk among ourselves and not publish anything--just do it by ourselves?" And somebody had scrawled on the side: "Commie." The idea of doing science in a commune and not publishing it seems absurd to me. Why should we get together to follow these pursuits which are not really pleasurable? Molloy had a certain relief and was satisfied that he had found a solution, but the main thing for him was that he was easy in his mind. As easy as one can be in a matter of this kind; suck them turn and turn about. I mean, he had to relieve the uneasiness of his mind. That's where the neurosis comes in--the obsession.

QUESTION: I've been uneasy without being able to articulate it very well, because it seems to me that you say something about the personal obsessions of scientists and the irrelevance of the goal or consideration of a moral principle in their work. I think it's probably only a half-truth. Einstein was a deeply moral man, very concerned. I have a feeling that scientists in their work are buoyed and reinforced by the belief that the answer to question 1 is "yes."

DELBRÜCK: Yes, of course you can be buoyed by the feeling that you've done society good; you can be buoyed by the feeling that you're acquiring fame and prizes. My point is this: Prior to these reinforcements, and more fundamental, even the old, decrepit beachcomber cannot avoid being a scientist, in an obsessive way (exactly the way Einstein was), although both the accessory components are missing. As for Einstein as a young patent clerk in Berne, in 1905, I doubt that he then made a connection between his physics and his responsibilities to society. That's the point I wanted to make. Thank you for making me point it out again. I mean these other components are there, of course, and if you read Jim Watson's book, The Double Helix, you might think that getting a Nobel prize is everything. However, this would be a grievous misconception.

QUESTION: How many scientists on a desert island would do science for their own benefit?

DELBRÜCK: Even Molloy would. But not for their benefit. He doesn't do it for his benefit. He does it compulsively. I think we all do. No, I take it back. Maybe not. It's a difficult question to answer because most of us are so dulled in our sensitivities that we may be quite incapable of any such complicated argument or reasoning or have the amount of relaxation that this man had. Of course, he had to be able to sit there for hours on the beach and dream up interminable martingales. If you put people on a desert island, probably quite a few of them would dream up interminable martingales and be satisfied with finding something that works.

QUESTION: I wonder if the one place where this parallel between Molloy and other scientists doesn't hold is that Molloy doesn't seem to have any intentions of communicating his results to anyone else. Do you think Einstein would have done his work if he had had no intention of publishing the results? And a personal question: Would you have done science if you had thought no one would be interested in the results?

DELBRÜCK: No, certainly not. In this first essay from which I quoted, by the Scripps girl, it said that scientists are playing animals. They not only play alone but they also play together, and if they are not too morose, they actually prefer to play together. And most scientists do prefer to play together. In the case of Einstein, of course, he would never have heard of the paradoxes of prerelativistic physics if he had not been in communication. No, a great joy of the business is communication. All I wanted to point out is the obsessive component of the immediate act of doing science. The channeling of this component toward the erection of a large structure, the institutionalization of it, that is a creation by society and that is something different. That is not a primary characteristic of Homo scientificus.

Recombinant DNA

The current controversy over research on recombinant DNA provides a particularly good example of the interplay between science and society, because there appears to be a conflict between scientists' need for freedom in pursuing their research and society's need to regulate potentially dangerous activities. Scientists have learned how to combine genes (DNA molecules) from different organisms and to produce many copies of a gene by growing large numbers of the appropriate organism in the laboratory. This recombinant-DNA technique is a new and powerful tool for investigating the machinery of the cell. It also has potential practical applications; for example, vats of bacteria containing the human gene for insulin could produce quantities of the hormone to the benefit of people with diabetes. However, some scientists and some individuals outside the scientific community have questioned the desirability of research on recombinant DNA because they fear possible harmful consequences such as creation of a new and dangerous organism. The government has regulated such research, and some communities have also attempted to regulate it. The specific case raises several general questions: What are the consequences of doing research? What are the consequences of not doing it? Who should decide what research to do?

To obtain an overview of the techniques involved in "gene-splicing" and the arguments surrounding their use, we recommend reading the July 1977 Scientific American article by Clifford Grobstein. Other sources of information on this topic are listed below.

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Biased Views of the Nature of Women

Some science-society interactions involve ideas, rather than tangible goods, but they shape our lifestyles as much as any technological progress. To illustrate this interaction, we have chosen to examine ideas about the nature of women. Our intention is not to represent comprehensively the ancient and continuing debate about male-female differences but to sample selectively from scientific studies of the nature of women. We use this issue to demonstrate that scientific observations and theories can influence the prevailing beliefs of a society and, conversely, that scientists are influenced by the society in which they work.

The use of "scientific" data to reinforce opinions originally derived from the philosophy and theology of a particular society is readily seen in nineteenth century debates about "woman's place." These scientific debates were not only theoretical but had practical consequences for the role of women in society. As one example, the influence of biological science on Victorian society is discussed in the article "Sex, Science, and Education" by Janice L. Treckler.

The reciprocal interaction, the influence of society on its scientists, can bias the selection of problems for investigation, the phrasing of research questions, and the conclusions drawn. The importance of asking the right questions about male-female differences is discussed in the article by Margaret Mead. Dr. Mead also describes the methods used by cultural anthropologists to understand human nature.

Particularly interesting questions can be asked about the origin and development of differences in the behavior of males and females. By comparing various human societies we have learned that what is appropriate behavior for females in one culture--for example, doing strenuous physical work--may be considered inappropriate in another. Such cross-cultural studies can help assess the extent to which human behaviors are genetically determined or are learned from the society. An example of contemporary research on this topic is provided in the article by Barry, Bacon, and Child. This comparison of different cultures suggests that boys and girls, and men and women, differ in their behavior primarily because they have learned what is appropriate, and not because they are innately very different.

In addition to comparisons of male-female behavior, anthropologists may study the status of women in different societies, their relative contributions to the economics of the society, and the roles women have played in the evolution of human beings and their societies. It is difficult if not impossible to examine these questions without bias arising from the scientists' own cultural perspective. Unavoidably the anthropologist is time-bound as well as culture-bound. Anthropologists or the popularizers of their observations may also be influenced by an unwitting assumption of the male bias that has permeated their society and therefore their science. Relatively recently, individuals with feminist viewpoints have attempted to counter the Western male bias in anthropological work. The article by Sally Slocum provides one example of the way in which traditional male-biased theories can be reinterpreted from a feminine perspective.

Questions of bias are closely tied to questions about words that may be loaded with emotional inferences. As a final topic for this subsection, we consider how views of women's roles in our society are subtly shaped by language. Since scientists, like others, use this biased language to formulate their questions, interpret their data, and report their results, the societal influence on science through language is particularly pervasive and difficult to counteract.

Biased Science in Society

As you read the following articles, look for examples of culturally determined biases and value systems in the scientific work of anthropologists and biologists. In particular, consider the following questions: What was the "evidence"? What conclusions were drawn from the observations? What other conclusions or interpretations could be derived from the evidence? How did the interpretations of the same observations change with time or with new observations? To what extent are your own views of the nature of women conditioned by scientific findings from this or previous centuries?

SEX, SCIENCE, AND EDUCATION

by Janice Law Trecker

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It was a widely held nineteenth-century belief that science was a liberating intellectual force. The scientific method and the new scientific theories were not seen simply as means of exploring nature and matter, but as tools for approaching moral and social problems as well. The powers of the scientific method were believed sufficient to sweep away old prejudices and misconceptions while building up a new social and intellectual order. No social or even political problem was judged too great or too complex for this new approach and, especially in the last quarter of the century, scientists turned to their disciplines for the solution of a variety of vexing social problems. One of these harassing questions was the "Woman Problem," and rejecting old moral and theological data, scientific minds resolved to turn their expertise to a study of women and of women's place: "The question of woman's sphere, to use the modern phrase, is not to be solved by applying to it abstract principles of right and wrong. Its solution must be obtained from physiology, not from ethics and metaphysics. . . . The quaestio vexata of woman's sphere will be decided by her organization," wrote a prominent physician in 1874. (1) The Quarterly Journal of Science felt that Darwinism suggested a sound approach to the Woman Question: woman's role could be illuminated by applying the master's ideas of natural selection, differentiation and specialization to the study of social and sexual relationships. (2) While intellectuals might disagree on which scientific theory might best apply, there was never any doubt as to who would do the studying, examining and theorizing.

G. Stanley Hall wrote in his monumental Adolescence that woman was unable to solve her own problems or to be her own teacher, preacher or

doctor: therefore, "she must be studied objectively and laboriously as we study children, and partly by men, because their sex must of necessity always remain objective and incommensurate with regard to woman and therefore more or less theoretical." (3)

This belief in the sovereign objectivity of science concealed certain difficulties which were to invalidate the scientific answers to the "Woman Question." These difficulties were, first, the tendency of inherited, non-scientific ideas to penetrate the scientists' social theories and, second, the fact that science, intellect and education were in themselves points of conflict between the new aspirations of the women's movement and the attitudes of the social order.

Perhaps it was the theoretician's very confidence in his own professionalism which permitted the easy transition from old stereotypes to "scientific" formulations. Hall's emphasis on masculine objectivity with regard to women too often became a purely theoretical stance. Despite their interest in the Woman Question, many of the thinkers who wrote about it had little interest in studying the feminine. Rather their interest was to fit woman into a variety of biological, evolutionary and anthropological theories. Most of these hypotheses were characterized by a dualistic bias which saw the female in all ways as the complement and contrary of the male. This prescientific habit of mind, combined with a belief in the objectivity of research and the purity of scientific study, permitted conservative nineteenth-century doctors, scientists and educators to rework earlier theories about women's rights and place, without realizing that the origin of their ideas lay not in their empirical data, but in their cultural heritage. They ransacked the works of the evolutionists, comparative biologists, anthropologists and physicians. They measured skulls, weighed brains, charted aptitudes for mathematics and tested memory; they figured out the average male consumption of food and studied the lives of individuals of genius. When they finished and made their recommendations for the proper role and education of women, however, they spoke with St. Paul and Rousseau about the complementary physical and psychological nature of the sexes and the subordination of women.

The new social and symbolic importance of science further complicated the situation and eventually made the scientist's quest for objectivity quixotic. This was especially true regarding theories about women and education, since science had succeeded theology and philosophy as knowledge "forbidden" to women and other inferiors. When classical languages and theological and philosophical studies had led to power and advancement within the masculine hierarchy, these studies had been believed too hard for the delicate structure of the female brain. In religious ages especially, tampering with theology had been considered inappropriate and "unfeminine." However, both the intellectual focus and the centers of social power had shifted by the nineteenth century. Whatever the pious might think, it was no longer theology or a mastery of Greek and Latin that prepared a young man for a brilliant career. Ancient verb forms and scholastic disputes could not match a knowledge of manufacturing, technology or engineering in an industrial era. Science and mathematics were displacing classical and philosophical studies, and with

them came reasons why women should not be highly educated, at least not in such "difficult" disciplines as the sciences.

Nowhere is this shift in what constituted forbidden knowledge more clearly illustrated than in medicine. In the medieval world woman was the physician, and long after, the housewife's simples and potions and her nursing skill largely determined the health and safety of her family. At the dawn of the industrial era, women still dominated one medical specialty, obstetrics and gynecology, and the midwife was a respected figure in every American community. Once medicine became a science, however, attitudes changed, and even the midwife was driven from the field by professionally educated male practitioners. Suddenly medical matters were improper for women to know, and a decent, prudish ignorance plus a deferential confidence in the male physician became the correct female attitude. (4)

Science's new designation as "masculine" knowledge, and male fears of economic competition, made many of the theorists' arguments against female knowledge seem self-serving. No one could overlook the fact that science and the scientific method were potent as symbols as well as tools. The emotional connotations around the study of science and the belief in its great practical power made it almost impossible for scientists to be unbiased. Despite their confidence in their complete objectivity, the nineteenth-century scientists' analysis of the Woman Problem forms a striking case study of the ways in which nonscientific ideas and social needs shape scientific thinking.

This is particularly true of the scientists' approach to women's education, the focus of much of their work on the Woman Problem. Education was a primary interest for several reasons. First, like other Americans, the nineteenth-century scientific thinkers believed in its power. They tended to see the solution to many social problems in terms of education, and they had faith that the right sort of woman--or man--could be produced by the correct method of education. Second, they saw the growing body of educated women as direct professional and economic competition. While this was, of course, rarely advanced as a reason for desiring changes in women's education, the scientists' insistence that the "hard" sciences and medicine should not be a part of the feminine curriculum would, if followed, have kept many vital social, intellectual and industrial processes a masculine preserve.

Finally, education attracted attention as the most successful feminist challenge to the old ways of thinking. The nineteenth-century women's movement potentially threatened great changes in education, in social and sexual mores and in politics. In the last quarter of the century, however, only the first of these really showed striking progress. It is significant that the scientists' analysis of and attacks on women's education came not when the pioneer academies were struggling for survival or when women were first integrating higher education. Rather, the scientific thinkers brought forward their new solutions to the Woman Question after women's education, including the study of the sciences, was well advanced and indubitably a success. No matter what their discipline or their initial approach, the scientific conservatives showed an interest in altering

women's education. It is probable that this interest in education stemmed as much from the fact that feminist success was most evident in this field, as from a conviction as to the importance of education in shaping the feminine character.

Because they wrote after the development of higher education for women, the new theorists differed from older anti-feminist writers on women's schooling. The "scientific" arguments took account of women's new intellectual achievements--sometimes even conceded her intellectual equality--but they cautioned that these achievements were dangerous: for the woman's health, for the survival of the race, for the continued progress of the species. Furthermore, the new theorists against the intellectual female raised another, more subtle barrier to women's achievements and expectations. At the time when women were seeking to open professional and postgraduate education and to enter the learned professions, scientists brought forth evidence to indicate that woman was incapable of true excellence and that only a mediocrity of achievement and a supportive role in discovery could be her portion. Like Rousseau, these scientists believed that man was destined for discovery and innovation, woman for application and conservation.

The resulting arguments against the intellectual woman are difficult to classify, because they drew from an assortment of nineteenth-century obsessions. Evolution, the survival of the fittest, the racial supremacy of Caucasians, race suicide, the home, the dream of an orderly society--all these hopes and fears and cultural properties were mixed up with the role of woman, the psychology of the female and the proper ordering of the relations between the sexes.

A sexist and racist view of the world was at the heart of many nineteenth-century scientific, as well as social, assumptions. Biology, anthropology and psychology were not immune to the idea that progress was rooted in the triumph and rule of superior species and beings. In scientific, as in popular understanding, the superior human being was identified with the white American or European male of a certain social standing, and anything which threatened the contemporary social order was seen as detrimental, not only to the status quo, but to the very progress of humanity. (5)

The first danger which women's education posed for stability and progress was the deterioration in health which rigorous intellectual work caused female students. While not the first to advance this thesis, Dr. Clarke's Sex in Education was one of the most influential. His argument, and those of such contemporaries and successors as G. Stanley Hall, Henry Maudsley, T. Clouston and James Crichton-Browne, was based on two quite remarkable medical presumptions. (6) First, that woman's functional periodicity, i.e., menstruation, was the dominating fact of her life, and, second, that the mind-body linkage was such that the principle of conservation of energy should be applied. According to this view of mind and body, the brain withdrew nourishment from the rest of the body during intellectual activities. If the brain was overworked, often the body suffered and the individual's health decayed, frequently in the direction of hysteria and neuralgia.

Of course, such a mind-body linkage would apply to both sexes. However, because of menstruation and other facets of the female reproductive life, the withdrawal of "vital energy" by the brain was seen as more dangerous to women, especially at puberty, when the reproductive system was developing. Not only did menstruation cause women to be more susceptible to "derangement" and "instability," but neglect of rest and an excess of mental strain could permanently damage the constitution and the reproductive system.

Damage could follow directly from the brain's drawing too much blood from the reproductive organs, or indirectly through the evil effects of tension brought on by overly rigorous work. It could occur during a girl's school days, or after she finished school and was ready to begin her home and family. In either case, the ill effects were manifold. In 1892 Sir James Crichton-Browne, a British M.D., cited a host of ills including insomnia, neuralgia, chorea, anemia, hysteria and "general delicacy" among the effects of current methods of educating girls. (7) T.S. Clouston added warnings of stunted growth and nervousness, which was in turn related to headaches and hysteria. (8)

Considering the dangers these doctors saw in any activity which depleted the body's energies during adolescence, one would have thought they would have turned their primary attention to the severely overworked girls of the lower classes--to the factory children and the young domestics, seamstresses and farm workers. This, however, was not the case. It was intellectual, not physical application which interested this segment of the medical profession. Clarke, who was one of the few even to consider the plight of the young working woman, dismissed her dangers. The woman of the working class risked less since the real strains of physical labor typically didn't begin until after puberty (a dubious assumption in the 1870s) and in any case, she was not apt to be developed intellectually. What energy she had went entirely to the development of her physical organization instead of being frittered away in the study of Latin, Greek or advanced mathematics. (9)

The decay in health of intellectual, upper-middle-class female students was not, however severe, solely their own misfortune; it threatened their posterity as well. Because it was theorized that intellectual stimulation particularly affected and interrupted the reproductive system, education was thought to have a direct effect on the fecundity of graduates. Dr. Henry Maudsley wondered if highly educated women would have any children at all; Dr. Clarke predicted that in another generation American men would have to import wives. Hall, too, placed the blame on the college woman and not the college man: ". . . for she is far more liable than he to overdraw her reproductive power and consume in good looks, activity of mind and body, and other augmentations of her individuality, energy meant for the altruism of home and of posterity." (10)

Hall's use of the term "individuality" was significant. It was not only that the development of woman's individuality was threatening psychologically, but that individuality had a theoretical significance in contemporary biological theories. As Hall explained: "The bachelor woman is an

interesting illustration of Spencer's law of the inverse relation of individuation and genesis. The completely developed individual is always a terminal representative in her line of descent. She has taken up and utilized in her own life all that was meant for her descendants and has so overdrawn her account with heredity that, like every perfectly developed individual, she is also completely sterile. . . . While the complete man can do and sometimes does this, woman has a far greater and very peculiar power of overdrawing her reserves." (11)

The segment of the scientific community represented by Crichton-Browne and Clarke was chiefly concerned with the individual woman and with the children they felt she wanted to bear (or should want to bear). The theoreticians of the science establishment, their popularizers and the social theorists were more concerned with the impact of low fecundity on the well-being of the community. British author Grant Allen, for one, was impressed with Darwinian ideas about the importance of an increasing population for the maintenance of animal vigor and for the selection of a superior stock. Since his own calculations convinced him that the British population would only remain stationary if each woman had four children, or if each marriage produced six, Allen was thoroughly alarmed by the declining birthrate. With rare boldness, he declared that married or not, women had to produce children; the alternative was the enfeeblement of the "cultivated classes," whose fertility had already declined sharply by 1889. (12)

While Allen's concern with the "cultivated classes" reflected the British situation, American writers were equally concerned that the "best" stock should remain dominant. Alarmists cited not only the high birthrates of the races of Africa and Asia, but the birthrates of immigrant stocks in America. (13) They saw the continued dominance, indeed existence, of their own group imperiled by the sudden shift in female attitudes, and with Allen, they agreed that women's "sexuality (which lies at the basis of everything) is enfeebled and destroyed." Education for women must therefore be shifted from the intellectual, masculine pattern to something new. As Allen wrote, ". . . instead of reform taking a rational direction--instead of women being educated to suckle strong and intellectual children, and to order well a wholesome, beautiful, reasonable household--the mistake was made of educating them like men--giving a like training for totally unlike functions." (14)

The idea that education was a detriment to female fertility was not the only nineteenth- and early twentieth-century theory about women to draw support from evolutionary doctrines. There was another set of arguments placing limits on the attainments of the educated female which was also sustained by ideas drawn from the biological sciences. In this view, male superiority was a direct and inevitable consequence of greater genetic variability, which resulted in a greater possible range of masculine talent. This thesis was supported by a variety of arguments from comparisons of brain weights to complex evolutionary theories and subtle psychological investigation. Much of the research and speculation concentrated on the brain, as crude statements of female inferiority based on brain size gave way to consideration of the differences in intellectual and anatomical

variations shown by each sex and of the possible significance of the differentials.

As Havelock Ellis pointed out in *Man and Woman*, few studies have more clearly shown the weight of unscientific prejudice than the study of the brain. (15) Indeed, to the probable discomfiture of those who based social arguments on male variability, it had at first been believed that female brains showed greater variability, a circumstance identified with the inferiority of woman's more primitive organ. Later, as it appeared that the male brain was the more variable, this interpretation changed. The newer view held that it was variability of intellect (suggested by variability in physical as well as mental abilities) which gave males the edge in human society. As the pioneer psychological tester Edward Thorndike wrote: "This one fundamental difference in variability is more important than all the differences between the average male and female capacities. . . a slight excess of male variability would mean that of one hundred of the most gifted individuals in this country not two would be women, and of the thousand most gifted, not one in twenty. . . . Not only the probability and desirability of marriage and the training of children as an essential feature of woman's career, but also the restriction of women to the mediocre grades of ability and achievement should be reckoned with by our educational systems." (16)

Like a number of other scientific commentators on the education of women, Thorndike was forthright in drawing conclusions from his theoretical assumptions. "The education of women for such professions as administration, statemanship, philosophy, or scientific research, where a very few gifted individuals are what society requires, is far less needed than education for such professions as nursing, teaching, medicine or architecture, where the average level is essential." (17) While high school and even college education was possible and acceptable for women, postgraduate education was socially a more "remunerative" investment for males.

This sort of social and educational conclusion was frequently drawn from ideas about male variability, a habit which one believer in the theory, Havelock Ellis, criticized on the grounds that the ability of the sexes was comparable, except at the furthest ends of the scale. Despite his caution about drawing sweeping educational prescriptions from variability, Ellis himself made use of the idea to account for the prevalence of male genius. Denying the existence of any social and cultural restrictions on feminine ability, Ellis theorized that while not totally restricted to males, genius was so rare among women as almost to be counted a masculine secondary sex characteristic. (18)

Ellis, Thorndike and other psychologists, biologists and physicians drew support from a variety of studies and statistical investigations, including relative numbers of male and female feeble-minded, extensive measures of brain weights and cranial capacities, anatomical measurements of infants and studies of geniuses. Although their facts and figures were plentiful, their conclusions were hotly debated. Not only was the jump from greater male physical variability (even in such suggestive areas as

brain weight) to greater mental variability questioned, but a variety of methodological objections were raised as well. Critics like Leta Stetter Hollingsworth cited social factors influencing crucial data and pointed to widely differing interpretations of terminology and statistical procedure. (19) Dr. Karl Pearson, who denied that the sexes differed in their degrees of variability, conducted a running battle with Havelock Ellis over methods of taking anatomical measurements and over what could legitimately be considered a variation. (20)

Considering the uncertainties surrounding the facts about male variability, it is surprising that this idea had such marked appeal. However, in addition to flattering old prejudices, the notion of variability was sustained by its role as a small but integral part of the theories of evolution.

One of the things which had interested Darwin and other biologists and evolutionists was the means by which new traits appeared. Among their objects of study were domestic animals, since selective breeding accelerated the production of new traits. Their observations led them to believe that new traits were more frequently introduced in males and transmitted through male offspring. Combined with the fact that in most feral mammal and bird species the adult male differs more from the young than does the adult female through such secondary sex characteristics as horns, brilliant plumage and manes, this evidence suggested that the male is the agent of variation in evolution. According to Darwin, there was both a psychological and a physiological basis for this tendency: "The greater eagerness of the males [i.e., their sex drive] has thus indirectly led to their much more frequently developing secondary sexual characters than the females. But the development of such characters would be much aided, if the males were more liable to vary than the females--as I concluded they were." (21)

Darwin's idea of the variability of males, combined with his belief in stereotypical psychological characterizations of the sexes, was further elaborated by W.K. Brooks, who thought that males had a unique ability to store up variations within their reproductive cells through "gemmules." (22) Postulating gemmules did not really describe the genetic mechanism at work, but Brooks, at least in his popular articles, vigorously clarifies what this mechanism might mean with regard to the Woman Problem. Believing that evolution was a combination of variation and heredity, Brooks advanced the idea that the ovum is "the material medium through which the law of heredity manifests itself, while the male element is the vehicle of new variations. The ovum is the conservative, the male element the progressive or variable factor in the process of evolution of the race as well as in the reproduction of the individual." (23) Furthermore, there was what Brooks called the "subjective side of evolution." For, he wrote, ". . . if the female organism is the conservative organism, to which is entrusted the keeping of all that has been gained during the past history of the race, it must follow that the female mind is the storehouse filled with the instincts, habits, intuitions, and laws of conduct which have been gained by experience. . . . The male mind must have the power of extending experience over new fields, and by comparison and generalization, of discovering new laws of nature." (24)

It is not too surprising that Brooks drew conservative conclusions about women's education. The proper education for women was general cultural knowledge to advance their role as the storehouses of past ideas. Only males should receive the technical and scientific training which is a prerequisite for new discoveries.

The female role as racial conservator strengthened the prevalent opinion that it was easier to describe (and prescribe for) the typical woman than the typical man. G. Stanley Hall's effusion on woman as "representative of the race" is instructive: "The male is the agent of variation and progress, and transmits variations best. . . . An ideal or typical male is hard to define, but there is a standard ideal woman. Because her mind is, more than that of man, essentially an organ of heredity, we find that, although she may seem volatile and desultory, the fact that her processes seem to be unconscious emancipates her from nature less than is the case with man. Her thought is a mode of thinking." (25)

In the terminology of the times, woman was anabolic--conservative and natural, while man was katabolic--variable, occasionally destructive, but able to go beyond nature, in thought as well as in physique. The Victorian stereotype of femininity was not, therefore, simply one cultural choice out of many, but the paradigm of the feminine role in the great drama of evolution. The woman who in some way challenged the stereotype was not only out of step with her culture, she was out of step with the very pattern of the universe. In the much quoted dictum, "The differences [between the sexes] may be exaggerated or lessened, but to obliterate them it would be necessary to have all the evolution over again on a new basis. What was decided among the prehistoric Protozoa cannot be annulled by Act of Parliament." (26)

Furthermore, it was not only a law of progress that there be sex differences, but that differentiation by sex steadily increase. This "law" was suggested by the discovery by comparative anatomists that the differences between the sexes in cranial capacity were more marked in "civilized" and "developed" stocks than in the "lower," more "primitive" races. Researchers pointed to new data which implied that there were greater differences in skull capacity between the sexes in European than in African stocks and greater differences between modern males and females than between the sexes in ancient stocks. (27) Since there was no doubt in the Western mind that the modern European or American represented the highest point of human development, the conclusions were clear. The noted "brain inferiority" of the female was a law of nature and the increasing differentiation and specialization of the sexes was an instrument of progress. Rather than striving to make women like men, a true social ordering and proper education would seek to accentuate the differences and to make the sexes mutually dependent and complementary--a process which would, incidentally, also increase sexual attraction.

The educational suggestions which these evolutionists drew from their theories were like those of the conservative medical men. The ideal female curriculum aimed at giving women the graces of a liberal, humanistic education without the intellectual tools or the spiritual hardiness to do much more

than appreciate the productions of masculine genius. In the inimitable style of Dr. Hall: "Another principle [of female education] should be to broaden by retarding; to keep the purely mental back and by every method to bring the intuitions to the front; appeals to tact and taste should be incessant." If appeals to "tact and taste," emphasis on the heroic and spiritual elements in history and the encouragement of a sentimental imagination were not enough, girls were also to be burdened by an oversensitivity to their physical organization: "... perhaps the deepest law of the cosmos celebrates its highest triumphs in woman's life. For years everything must give way to its thorough and settled establishment. In the monthly sabbaths of rest the ideal school should revert to the meanings of the word leisure . . . then woman should realize that to be is greater than to do." (28)

Woman's education should be primarily "education for motherhood," and while the conservatives of the nineteenth century were willing to allow some intellectual training for females, they certainly did not want women who could enter their own intellectual preserve. "The savante--the woman of science--like the female athlete, is simply an anomaly, an exceptional being, holding a position more or less intermediate between the two sexes. In one case the brain, as in the other the muscular system, has undergone an abnormal development," declared the Quarterly Journal of Science. (29) Dismissing the idea that a previous denial of training had anything to do with the absence of first-rate female scientists, the Journal continued, "We would not seek to entice women into the observatory, the laboratory, or, above all, into the dissecting room, nor erect colleges for the training of female savantes."

However expressed, the underlying aim of the scientific conservatives was to suppress and to direct female individuality and talent into the socially acceptable channels of domestic, idealized womanhood and away from the disciplines which might increase her dangerous individuality and her knowledge of the crucial studies--i.e., the sciences. Having so recently secured the individuality of their own sex through such novelties as male suffrage, the nineteenth-century rise in affluence, mass education and the escape from subsistence farming, conservative men were uncertain and frightened of the next step. In the new Faustian bargain, the masculine half of humanity would be given individuality and power through science, technology and abstract thought, while the feminine half of humanity would, in exchange, remain tied to nature, the race and reproduction. (30) No longer was his pattern the farmer, tied to home and soil, but the industrialist, the worker, the craftsman, the scientist, dependent not on nature but on the new mysteries of trade, capital and technology. This new masculine freedom would have to be purchased, and the price was the sacrifice of woman's individuality. Her willingness to be submerged in duties for the sake of the family, the state and the species was the condition of his emergence as the new man of the nineteenth century.

The conservatives elaborated their ideas with considerable imagination and, however inaccurate and unscientific they may now appear, they were impressive in their union of the interests and fears and theories of their own day. By relating social and ethical questions to a supposedly

impersonal science through evolution, these theorists could claim a consistent biological and philosophical foundation for their ideas about society and government. In the case of their ideas about women, a highly traditional formulation of women's proper role and education was seen as congruent with the feminine role in the family, the state, the species and even in the overall working out of the basic principle of the universe, the unfolding evolution of beings. Such a synthesis was not to be taken lightly.

Nonetheless, the speculations of the scientists were never allowed to dominate the field, and just as their analyses concentrated on education, so did the rebuttals of their opponents. Clarke's Sex in Education brought down a storm of protest, including the able volume of essays, The Education of American Girls, published specifically to counteract its influence. The conservatives were opposed on scientific, factual, methodological and philosophic grounds. To refute them, girls' health and academic records were scrutinized with the closest eye, records of fertility and health were kept, and the effects of a variety of educational practices detailed. (31) In spite of diversity of the factual material assembled, however, the champions of women's education were united on their main objections. The women and men who wished to secure not only education, but real individuality for their female pupils rested their philosophic case on a dynamic conception of the female mind and on a new awareness of the social factors which influence the development of the human personality. For neither of these, but especially not for the latter, have they received adequate credit.

The arguments advanced by the moderate and progressive educators, the ideas of the feminists and the data on the health and achievement of American female students eventually destroyed fantastic fears about advanced mathematics causing sterility and science destroying the femininity of America's students. What is surprising is the tenacity of the "scientific" arguments, not their eventual defeat. Thomas Woody, the pioneer student of the history of American women's education, wrote: "It is strange, considering the immediate and very evident success of women in doing college work, that this belief in their mental inferiority and physical weakness continued as long as it did. Its persistence in the face of facts was one of the best proofs of the social prejudice that opposed women's collegiate education." (32)

In the case of the scientific and medical arguments, however, it is probable that they were sustained not only by the weight of antique prejudice, but also by the fact that prejudice was stated in the most modern and approved terms. While a priori statements of feminine limitations might have been laughed out of the journals and while religious sanctions were losing their power, arguments drawn from scientific research and especially from the data and theories of evolution had irresistible conviction. Modern researchers trying to account for the still powerful prejudices against the intellectual woman and against equal opportunity for women in education are often surprised at the strength of ideas ultimately derived from Genesis and the Epistles of St. Paul. The crucial point, however, is that those ideas saw a number of reincarnations--as medical cautions, as social theories and

as appendages to evolution. A recent article on the history of American sociology has suggested that ideas about female inferiority in the work of pioneer American sociologists helped prepare the way for a rapid acceptance of similar ideas in the psychologies of Freud, his successors and popularizers. (33) An even stronger case can be made for the preservation of ideas about female inferiority within the educational and medical communities, and for the transmission of nineteenth-century limits on women's individuality and intellect through the scientific and pseudo-scientific writings on women's education and "place."

While conservative scientists lost the battle against higher education for women, it is by no means clear that their influence was destroyed. Many of the problems of female education today--its reinforcement of sexual stereotypes, its subtle discouragement of the full range of feminine talent, the pressures on girls to avoid science, mathematics and technical subjects, and the well-documented bias against females in the upper levels of education--are indications that beliefs about the desirability of limitations on female education have not been totally destroyed, but have simply reappeared in different intellectual and social clothing. (34)

Footnotes

- ¹Edward H. Clarke, Sex in Education (Boston: Osgood, 1874), p. 12.
- ²Anon., "Biology and Women's Rights," Quarterly Journal of Science, repr. in Popular Science Monthly 14 (Dec. 1878), 201.
- ³Adolescence (New York: Appleton, 1909), 2:634.
- ⁴Alice Clark, Working Life of Women in the Seventeenth Century (London: Dutton, 1919), pp. 281 ff. for physicians' efforts to circumscribe midwives' work, knowledge and influence. Eleanor Flexner, Century of Struggle (New York: Atheneum, 1970), pp. 115-19, relates efforts to open medicine to women. For doctor-patient relations in nineteenth-century America, see Ann Wood, "The Fashionable Diseases," Journal of Interdisciplinary History 3 (Summer 1973), pp. 25-52.
- ⁵Other discussions of sex and race bias: Carroll Smith-Rosenberg and Charles Rosenberg, "The Female Animal: Medical and Biological Views of Woman and Her Roles in Nineteenth-Century America," Journal of American History 60 (Sept. 1973), pp. 350-52; Julia and Herman Schwendinger, "Sociology's Founding Fathers: Sexists to a Man," Journal of Marriage and the Family 33 (Nov. 1971), 583 ff.
- ⁶Other discussions: the Rosenbergs and Willystine Goodsell, The Education of Women (New York: Macmillan, 1923), pp. 63 ff. Interestingly, Goodsell was discussing these medical theories as still troubling questions.
- ⁷"Sex in Education," Popular Science Monthly 41 (Sept. 1892), 176.

⁸"Female Education from a Medical Point of View," Popular Science Monthly 24 (Dec. 1883), 214 ff. and 24 (Jan. 1884), 319 ff.

⁹Clarke, p. 131.

¹⁰Hall, 2:588.

¹¹Hall, 2:633.

¹²Grant Allen, "Plain Words on the Woman Question," Popular Science Monthly 36 (Dec. 1889), 170-81.

¹³Nathan Allen, "Changes in New England Population," Popular Science Monthly 23 (Aug. 1883), and "The Decay of the Family," N.Y. Observer, June 15, 1882. For later treatments: Arthur Newsholme, The Declining Birthrate (New York: Moffat Yard, 1911), and Sir Leo Chrozza Money, The Peril of the White (London: W. Collin's Sons, 1925). Also, John Higham, Strangers in the Land: Patterns of American Nativism, 1860-1925, rev. ed. (New York: Atheneum, 1971).

¹⁴Grant Allen, p. 175.

¹⁵Man and Woman, 6th ed. (London: A.C. Black, 1926), pp. 119 ff.

¹⁶"Sex in Education," Bookman (1906), quoted in Leta Stetter Hollingsworth, "Variability as Related to Sex Differences in Achievement," American Journal of Sociology 21 (Jan. 1914), 510 (Emphasis in original).

¹⁷Thorndike, p. 510.

¹⁸Ellis, pp. 430-31.

¹⁹Hollingsworth, pp. 513-23; Goodsell, pp. 72-76.

²⁰Goodsell, pp. 69-71; Ellis, pp. 527-46.

²¹Charles Darwin, The Origin of Species and The Descent of Man (New York: Modern Library, n.d.), p. 580.

²²Patrick Geddes and J. Arthur Thompson, The Evolution of Sex, ed. Havelock Ellis (New York: Scribner, n.d.), p. 267.

²³W.K. Brooks, "The Condition of Woman from a Zoological Point of View," Popular Science Monthly 15 (June 1879), 150.

²⁴Brooks, p. 154.

²⁵Hall, 2:567.

²⁶Geddes and Thompson, p. 267

²⁷M.A. Hardaker, "Science and the Woman Question," Popular Science Monthly 20 (Mar. 1882), 577.

²⁸Hall, 2:639-40.

²⁹Anon., "Biology and Women's Right," repr. Popular Science Monthly 14 (Dec. 1878), 205.

³⁰This idea is implicit in the sharp nineteenth-century demarcation of masculine and feminine spheres and the insistence on the greater closeness of the female to nature and to both mental and physical-racial stereotypes.

³¹For results of the survey done by the Association of Collegiate Alumnae (1884) and other studies, see Goodsell, pp. 86-89; also Lucinda H. Stone, "Effects of Mental Growth," The Education of American Girls, ed. Anna C. Brackett (New York: Putnam's, 1874); John Dewey, "Health and Sex in Education," Popular Science Monthly 28 (Mar. 1886), 606-14; Charlotte W. Porter, "Physical Hindrances to Teaching Girls," Forum 12 (Sept. 1891), 41 ff.; Mary T. Bissell, "Emotions vs. Health in Women," Popular Science Monthly 32 (Feb. 1888), 504-10; Nellie Comins Whitaker, "The Health of American Girls," Popular Science Monthly 71 (Sept. 1907), 234 ff.; Lucy M. Hall, "High Education of Women and the Family," Popular Science Monthly 30 (Mar. 1887), 614 ff.

³²Thomas Woody, A History of Women's Education in the United States (New York: Science Press, 1929), 2:155.

³³Schwendingers, "Sociology's Founding Fathers," p. 789.

³⁴Sex bias against women in education has been exhaustively documented. For example, Discrimination Against Women, Parts 1 and 2 of Hearings Before the Special Subcommittee on Education Regarding Section 805 of H.R. 16098, Washington, D.C. 1970; also, A Matter of Simple Justice, the Report of the President's Task Force on Women's Rights and Responsibilities, April 1970.

Understanding Human Nature

The perceptions of scientists are unavoidably shaped by their previous experiences. The study of human nature is particularly susceptible to misinterpretation based on previous social experience that may color perception of the empirical data. Examination of cultural anthropology can thus provide a vivid example of bias and attempts to counteract bias in science. The importance of minimizing the effects of bias and of asking appropriate questions is discussed in the following article by Margaret Mead.

Dr. Mead is a noted and influential anthropologist. She began her field work in 1925 in Samoa and made her sixteenth expedition in 1973. In addition to her work on primitive societies, she has made pioneering studies of complex industrial societies including that of the U.S. She has also been at the forefront of interdisciplinary collaboration, both national and international, to address vital problems of ethics and public policy.

MALE AND FEMALE

by Margaret Mead

Excerpted from *Male and Female*. (Originally published New York: W. Morrow, 1949.) New York: Mentor Books, 1954, pp. 13-45.

I. The Significance of the Questions We Ask

How are men and women to think about their maleness and their femaleness in this twentieth century, in which so many of our old ideas must be made new? Have we overdomesticated men, denied their natural adventurousness, tied them down to machines that are after all only glorified spindles and looms, mortars and pestles and digging sticks, all of which were once women's work? Have we cut women off from their natural closeness to their children, taught them to look for a job instead of the touch of a child's hand, for status in a competitive world rather than a unique place by a glowing hearth? In educating women like men, have we done something disastrous to both men and women alike, or have we only taken one further step in the recurrent task of building more and better on our original human nature?

These are questions which are being asked in a hundred different ways in contemporary America. Polls and tracts and magazine-articles speculate and fulminate and worry about the relationship between the sexes. In the moving pictures beautiful girls in tortoise-shell spectacles

and flat-heeled shoes are first humiliated for competing with men, then they are forgiven, loved, and allowed to be glamorous only when they admit their error. In the advertisements on the billboards, men are now told how they, if they wear the right hat, may be the chosen one, the loved one--a role that used to be reserved for women. The old certainties of the past are gone, and everywhere there are signs of an attempt to build a new tradition, which like the old traditions that have been cast aside will again safely enfold growing boys and girls, so that they may grow up to choose each other, marry, and have children. The fashions bear the imprint of this uncertainty; the "new look" of 1947 partly captured the fleeting image of the mothers of a generation ago, the boys could again find the girls marriageable--as their mothers were--while those same girls gained a new femininity by suiting their swinging gait to the remembered feeling of ruffled skirts like those their mothers once wore. In every pair of lovers the two are likely to find themselves wondering what the next steps are in a ballet between the sexes that no longer follows traditional lines, a ballet in which each couple must make up their steps as they go along. When he is insistent, should she yield, and how much? When she is demanding, should he resist, and how firmly? Who takes the next step forward or the next step back? What is it to be a man? What is it to be a woman? . . .

The differences between the two sexes is one of the important conditions upon which we have built the many varieties of human culture that give human beings dignity and stature. In every known society, mankind has elaborated the biological division of labor into forms often very remotely related to the original biological differences that provided the original clues. Upon the contrast in bodily form and function, men have built analogies between sun and moon, night and day, goodness and evil, strength and tenderness, steadfastness and fickleness, endurance and vulnerability. Sometimes one quality has been assigned to one sex, sometimes to the other. Now it is boys who are thought of as infinitely vulnerable and in need of special cherishing care, now it is girls. In some societies it is girls for whom parents must collect a dowry or make husband-catching magic, in others the parental worry is over the difficulty of marrying off the boys. Some peoples think of women as too weak to work out of doors, others regard women as the appropriate bearers of heavy burdens, "because their heads are stronger than men's." The periodicities of female reproductive functions have appealed to some peoples as making women the natural sources of magical or religious power, to others as directly antithetical to those powers; some religions, including our European traditional religions, have assigned women an inferior role in the religious hierarchy, others have built their whole symbolic relationship with the supernatural world upon male imitations of the natural functions of women. In some cultures women are regarded as sieves through whom the best-guarded secrets will sift; in others it is the men who are the gossips. Whether we deal with small matters or with large, with the frivolities of ornament and cosmetics or the sanctities of man's place in the universe, we find this great variety of ways, often flatly contradictory one to the other, in which the roles of the two sexes have been patterned.

But we always find the patterning. We know of no culture that has said, articulately, that there is no difference between men and women

except in the way they contribute to the creation of the next generation, that otherwise in all respects they are simply human beings with varying gifts, no one of which can be exclusively assigned to either sex. We find no culture in which it has been thought that all identified traits--stupidity and brilliance, beauty and ugliness, friendliness and hostility, initiative and responsiveness, courage and patience and industry--are merely human traits. However differently the traits have been assigned, some to one sex, some to the other, and some to both, however arbitrary the assignment must be seen to be (for surely it cannot be true that women's heads are both absolutely weaker--for carrying loads--and absolutely stronger--for carrying loads--than men's), although the division has been arbitrary, it has always been there in every society of which we have any knowledge.

So in the twentieth century, as we try to re-assess our human resources, and by taking thought to add even a jot or a tittle to the stature of our fuller humanity, we are faced with a most bewildering and confusing array of apparently contradictory evidence about sex differences. We may well ask: Are they important? Do real differences exist, in addition to the obvious anatomical and physical ones--but just as biologically based--that may be masked by the learnings appropriate to any given society, but which will nevertheless be there? Will such differences run through all of men's and all of women's behavior? Must we expect, for instance, that a brave girl may be very brave but will never have the same kind of courage as a brave boy, and that the man who works all day at a monotonous task may learn to produce far more than any woman in his society, but he will do it at a higher price to himself? Are such differences real, and must we take them into account? Because men and women have always in all societies built a great superstructure of socially defined sex differences that obviously cannot be true for all humanity--or the people just over the mountain would not be able to do it all in the exactly opposite fashion--must some such superstructures be built? We have here two different questions: Are we dealing with a must that we dare not flout because it is rooted so deep in our biological mammalian nature that to flout it means individual and social disease? Or with a must that, although not so deeply rooted, still is so very socially convenient and so well tried that it would be uneconomical to flout it--a must which says, for example, that it is easier to get children born and bred if we stylize the behavior of the sexes very differently, teaching them to walk and dress and act in contrasting ways and to specialize in different kinds of work? But there is still the third possibility. Are not sex differences exceedingly valuable, one of the resources of our human nature that every society has used but no society has as yet begun to use to the full? . . .

We can recognize how our experience limits our questions by exploring the possible results of asking different questions. Suppose we ask: "Aren't women just as capable of performing activity x as men are?" Or the reverse: "Aren't men as capable of activity y as women?" Investigations of this sort usually lead to quantitative comparisons, in which it may be found that men are a little faster than women, or women a little faster than men, or that there is no difference. Or the answer may be a little more complicated, so that women are found to be slower but more accurate,

or men quicker but lacking in small-muscle precision for the particular task. Once we have given such an answer, then, in terms of our present culture, employers, or benevolent government agencies, or one sex pressure group will set to work to exploit these differences or to minimize them, in order to get better work for the same pay, or to invent a machine that will equalize the difference. But in either case the discovered differences suggest no new use of human resources. They merely increase or decrease the probability that men and women will work in the same factory; they will primarily be the background not for using differences constructively, but for inventing some methods for equalizing the differences so that they do not matter, or for pigeonholing individuals in one job rather than another.

This is comparable to what we are doing with individuals with different degrees of acuteness of sight and hearing. Eyeglasses and hearing devices are eliminating the overt differences in performance between individuals. Where keenness of vision was once a vivid ingredient in the personality of a hunter, or a blessed myopia part of the equipment of a poet, we now know the effects upon the personality of having to wear eyeglasses or a hearing device. By our inventive attempts to equalize the performance of individuals with different capabilities, we enormously increase the number of individuals whose functioning is externally comparable in society, but we disallow those subjective differences which might have also made a contribution to human civilization. The more we ask questions about sex differences only so that we can get rid of them, or exploit them quantitatively, the more likely we are to find ways of eliminating them, both as bases of inequality and waste in the world, and also as bases of diversification of and contribution to the world.

There are, of course, very many areas in which it is an obvious gain to ask just this kind of question and get a simple quantitative answer with which we can deal inventively. It is nonsense to debar women from activities where their physical strength is only slightly inferior to men's when some simple gadget would make them as efficient as men. It is particularly nonsensical when it is accompanied by elaborate social rationalizations about a task's being unwomanly, or--in the opposite case--unmanly. To the extent that all the members of a sex are automatically debarred from using their talents in some field in which a little objective research would show that they are perfectly fitted to contribute, it is obviously useful to society to break such myths down. If we recognize that we need every human gift and cannot afford to neglect any gift because of artificial barriers of sex or race or class or national origin, then one of the things we must know is where the assumed differences between the sexes are mere elaborations of unimportant differences that can be dealt with easily in this invention-conscious world.

There is perhaps an even more fundamental way in which the questions we ask about the sexes can affect the groundplan of the world that, willy-nilly, with searching of heart or in utter carelessness, we are now engaged in building. We can ask our questions about sex differences in such a way that we focus on the limitations that sex imposes, and so very directly upon the limitations that our mammalian nature imposes upon

us. When we focus on sex differences in terms of limitation, every word we say includes certain deeper assumptions. "How much protection from physical endeavor should a pregnant woman have?" "How much freedom from monotony should a healthy young man have?" "How much physical sex experience is necessary to mental health?" "How many opportunities should a small child--who after all should be likened to a bear-cub, biting and tossing among other cubs--be given to bite, kick, and tear things to pieces?" "And should small males have more such opportunities than small females?" "How much allowance should be made for the periodicities of a woman's life?" All such questions, digging deep into our new willingness to come to grips with our biological past, and to build a work that will make due allowance for the limitations imposed by our mammalian nature, embody a sense of limitation. They stress those concessions we must make because if we do not make them, then many prices--in health, in happiness of individuals and of societies, in the symmetry and beauty of cultural traditions, in the peace of the world--will have to be paid. We find ourselves constructing a picture in which what have come to be called basic human needs are seen as boundaries beyond which we dare not pass, man-holes that must be skirted, or trap-doors that may be opening beneath our feet. Building a world in which human beings can live, or bringing up an individual family, becomes a task rather like building a house with one's eye on the zoning laws and the provisions of the health department, and cooking a dinner with charts of "loss of essential vitamins while cooking" as one's only cookbook. However much one may add to the list of basic human needs, seeing life in terms of needs--at least in our present climate of opinion--means essentially accepting a human nature as a statute of limitation. We may be promised that if we will only explore, list, and carefully satisfy these needs--for vitamins and minerals and so many grams of protein, for rest and liquids and oxygen, for the release of tensions (this is undoubtedly the most correct way to describe making love when the need category is uppermost), for continuous contact with one sensorily identifiable human being throughout the first two years of life, and so on, and so on--then we will be able to build a good society, rear healthy individuals, develop and perpetuate cultural traditions that have no snags or distortions, no murky ambiguities or falsely clear alternatives, and produce a peaceful world.

But such solutions leave us feeling empty and discontented. One has only to watch the faces of an audience while this point is expounded, and expounded as it so often is by those whose optimism and affection for the human race are unbounded and undaunted, to feel that even as the picture of the healthy child, the good society, the balanced, sane culture, and the peaceful world are unrolled before the watching eyes, a pall descends upon the spirits of the listeners. For back of the promise of sturdy little legs protected from the age-old threat of vitamin-A deficiency, of men and women whose clear eyes and jaunty gait assure us that they suffer from none of the pains and penalties of celibacy, the reading posture which shows that an eye-examination is given every year as a routine measure to every schoolchild--back of these looms a sense of limitation. If we take human needs as our only clues to the world we wish to build, we find in our mouths a flat taste. It is not a stale taste, because proper diet, rest, and sex hygiene have taken care of that, but simply a flat

taste, and an insistent desire for the disallowed and so contrasting position--a desire for lips that "save themselves hungry and bite blessed bread."

For out of this demand that we examine our human nature, assess its needs, those of males and those of females, those of little males and little females, old males and old females, those of different physique or different rate of maturation, comes little positive drive. All the prescriptions are negative in the end: How not to get rickets, or be delinquent, or become a kleptomaniac, or get old maid's insanity. How not to be an aspiring dictator, or start a war, or be a nymphomaniac, or the torturing tyrant of a concentration camp. How not to be the wrong thing, how not to dream--perhaps not the wrong dreams, but anyway how not to dream dreams that instead of functioning nicely to drain off antisocial wishes spill over into the day, and form the background of poetry--of the wrong kind. All these utopian plans neglect the other side of the picture, however much they may seek to bring it in: that as human beings we are not only interested in not doing wrong and evil and destructive things, but have also always cared terribly about our possibilities for doing good and right, constructive and beautiful, things.

But the answer does not lie for us, I think, at this period in history, in flinging ourselves to the opposite extreme. We can find no solace and no guide in ascetic or ecstatic commandments that flout the recognized biological needs of our mammalian nature. We have too recently come to understand the intricate connections by which a broken heart becomes a blocked nerve-track or a hyperextended artery, by which an unpaid grocery bill becomes something that an X-ray can reveal in the stomach, and a forgotten girlhood memory, a severe attack of dysmenorrhea. We know that when a little boy starts preferring to eat paper rather than food, it is not wise to wait until he has eaten all the theatre-tickets at Grandpa's birthday-party before taking him to consult a child specialist. However much we may want to proclaim that a man does not live by bread alone, the results of the federal nutrition program that ordered bread reinforced and helped banish pellagra from the American Southeast are part, and a very good part, of the stock of beliefs by which we live. Modern Americans, and probably a good proportion of modern western Europeans and modern Chinese and Japanese, are no longer able to flout our ever increasing knowledge of the cunning of our nervous system, or the sensitivity of our skin-tissues, or the responsiveness of our gastro-intestinal tract. Pictures of a world in which the clergy could say wholeheartedly, "We are not interested in civilization, we are interested in colonizing heaven," may arouse a piercing nostalgia in all who love the Cathedral of Chartres, but by so doing they simply unman one more needed worker in the workshop of civilization. Those who give themselves today so wholeheartedly to the lovely and gracious task of colonizing heaven that they have no time for public-health regulations or building codes, by so doing cut themselves off from almost the whole band of colonists whom they would wish to lead gently into the eternal daylight of God's presence. Man's fitness for heaven, which is perhaps one of the best ways we have thought of to describe man's potentialities that he does not share with any other known mammal, is no simple formula that will make modern men and

women turn away from the dutiful attention to newly discovered ways of increasing man's fitness on earth.

But we need not accept any such dichotomy between earth and heaven, between bodily needs and spiritual capacities, between limitations and potentialities. The potter who works with clay recognizes the limitations of his material; he must temper it with a given amount of sand, glaze it thus, keep it at such and such a temperature, fire it at such a heat. But by recognizing the limitations of his material he does not limit the beauty of the shape that his artist's hand, grown wise in a tradition, informed by his own special vision of the world, can impose upon that clay. Only if the potter ceases to remember the clay, and thinks to construct his pot by magic formulas that will work while he sleeps, or he who ceases to see a vision in his mind's eye and thinks that forceps and rules of proportions can replace that vision, is trapped and a failure. Because we are mammals, and male and female mammals at that, we have limitations, and we must know them, provide for them, keep them safely in our habits, if not continually and boringly in our minds. There are certain things that men cannot do because they are men, and women cannot do because they are women: begetting, conceiving, carrying, bearing, and suckling the next generation are divided differently. As the bodies of the two sexes develop, to be ready for their different roles in reproduction, they have basic needs, some of which are shared, some of which are different even in little children. All through our lives, the fact that we are creatures who are made not only to be individuals, but to continue the human race, is a persistent, unavoidable condition that we must meet. Failure to do so may express itself in innumerable ways: in a peptic ulcer or a temper tantrum, in a quarrel or a bad poem. (A good poem requires far more elaborate explanations to account for its existence.) But if we ask the question only in this way, ask only what are the limitations set by sex, the unavoidable conditions, the inevitable prices paid, then we invoke and reinforce the conflict, the traditional false dichotomy between high and low, animal and spiritual, body and spirit.

So when we ask the very urgent questions we must ask about the differences and the similarities, the vulnerabilities and the handicaps of each of the sexes, we must also ask: What are the potentialities of sex differences? If men, just because they are men, find it harder to forget the immediate urgencies of sex than do women, what are the rewards of this more urgent remembering? If little boys have to meet and assimilate the early shock of knowing that they can never create a baby with the sureness and incontrovertibility that is a woman's birthright, how does this make them more creatively ambitious, as well as more dependent upon achievement? If little girls have a rhythm of growth which means that their own sex appears to them as initially less sure than their brothers, and so gives them a little false flick towards compensatory achievement that almost always dies down before the certainty of maternity, this probably does mean a limitation on their sense of ambition. But what positive potentialities are there also? If at each step we ask, consciously and articulately: What are the limitations, and what are the potentialities, the lower limits and the possible upper limits, of the fact that there are two sexes, and of the differences between them? then not only may we find

answers that are in themselves rewarding about the place of the sexes in our present changing world; we shall do more. We shall make a contribution to the belief that on every question involving human beings we must concern ourselves not only with limitations, and not only with aspirations and potentialities, but with both. We will increase our faith in our full humanity--rooted in our biological ancestry that we dare not flout--capable of rising to heights of which each living generation can only glimpse the next step in the ascent.

II. How an Anthropologist Writes

In the jargon of modern American committee life and of general responsible social relationships, a phrase has crept in in the last few years, "from where I sit." It is often said half-jokingly, and yet it implies a total change in point of view. As one adds with a grin, or a half-smile, or perhaps a little rueful twist to the mouth, "from where I sit," this is an admission that no person ever sees more than part of the truth, that the contribution of one sex, or one culture, or one scientific discipline that may itself cross both sex and cultural lines, is always partial, and must always wait upon the contribution of others for a fuller truth. This [text] is being written from the standpoint of a woman of middle age, of an American, and of an anthropologist. It is part of the whole argument . . . that women will see the world in different ways than men--and by so doing help the human race see itself more completely. Necessarily, an American writes of America differently, with a different degree of involvement, than would a foreigner. The special contribution of an anthropologist, however, needs more comment.

An anthropologist's materials of study are the behaviors of living peoples living together in ways that they have learned from their forebears, who shared common patterns of behavior. The anthropologist's laboratories are primarily primitive societies, small isolated groups of people who because of their geographical or historical isolation have remained outside the mainstream of history, and preserved special practices of their own that contrast vividly with behavior in large societies. The anthropologist does not study primitive peoples primarily to find out more about the origins of our present ways of behaving. The Eskimo and the Samoan, the Ashanti and the Cheyenne Indian, have histories as long as our own, only different. While a study of these primitive peoples may throw light on some of the relationships between a simple technology, an uncertain food supply, a small population, and other aspects of social life, in our thinking about social origins we can only use these relationships as suggestive; we can never be sure in which ways our own ancestral forms of society were similar. But we can study primitive peoples to provide us with the material with which to think about human behavior, to give us clues as to when and how certain behaviors are learned. We go to these primitive societies to find material on the limits beyond which society cannot deny man's biological inheritance, and also to find variations in human behavior that we might not otherwise be able even to imagine as possibilities. . . .

When it comes to the question of what is maleness and what is femaleness, how male children differ from female children, men from women, anthropologists have their own special way of going at the problem. We do not devise tests of indistinct color-forms in which men may see one kind of image, women another. We do not count the number of times that little girls built flat and cozy dwellings while little boys build towers. We do not line up groups of men and women and give them tests in tapping-speed or button-pressing. Nor do we take rats or guinea-pigs and inject sex hormones into them and watch the changes in their behavior. We do not make detailed studies of the patients who come into our city clinic with such deep structural abnormalities that it is impossible to tell to which sex they belong. Nor do we follow out the life histories of men who have decided to live as women, or women who have decided to live as men, to find what anatomical or endocrinological or psychological clues set them on these paths. All of these approaches to the question of how and where men and women differ are valid and important. Conclusions drawn from any one of these approaches must be checked against another.

But the anthropological approach is to go out into primitive societies without any too specific theories and ask instead open-ended exploratory questions. How do male babies and female babies learn their social roles in different societies? What types of behavior have some societies classified as male, what as female? What behaviors have they failed to treat as sex-typed? How like have some societies felt males and females to be--and how unlike? We do not ask, initially, whether there are particular sex differences in personality that are systematically, and regardless of culture, associated with masculinity and femininity, such as passivity, initiative, curiosity, a capacity for abstractness, an interest in music. We do ask how different peoples expect infants to behave, how they use the difference in sex to define difference in role, and how they succeed in evoking the expected responses.

Such an inquiry does several things. First, it clears away our whole weight of cultural preconceptions about men and women, whether they are the traditional insistence on innate differences or the more contemporary attempt to deny many differences that have been accepted historically. This frees the mind from using the sort of arguments that have been invoked for and against the feminist movement, from hazy reconstructions of idyllic periods of history when women ruled and all was peace, and from querulous arguments about why women have not been great composers of music. The thousand and one statements that are made every day--"Isn't that just like a man?" "Men are just big children." "Women have such childlike minds." "Women are more sensitive than men." "Men are more sensitive than women." "Women are more variable than men." "Men are more variable than women"--have been swept from the mind like tattered autumn leaves from the garden-paths before it is possible to think clearly at all. . . .

Back of the word "man" stand in my mind a phalanx of images, men with white skins and brown skins and yellow skins and black skins; men with crew-cuts and shaved heads and great psyche knots; men in evening-dress in contemporary society and men wearing nothing but

gleaming pearlshell crescent ornaments on their chests; men with muscles that bulge and ripple and men with arms as slender as a girl's; men whose fingers are too clumsy to hold a tool smaller than an adze and men who sit threading tiny beads on a string; men whose manhood is offended by the smell of a baby and men who cradle an infant gently in a steady arm; men whose hands are always ready to fling upward and backward as if to throw a spear and men whose hands press easily palm to palm in a gesture of apology and entreaty; men of six feet six and men of five feet one. And beside them stand women, again with many-colored skins; some with bald pates and some with long flowing hair; women with breasts that hang very low or can sometimes even be trained to a form which can be thrown over their shoulders and women with small high breasts like the figures on the Medici tombs in Florence; women who swish their grass skirts as they walk and women who handle these same grass skirts as if they were sheets of iron protecting their virtue; women whose arms look empty without a child in them and women who hold their children at arm's length as if they were clawing little wild cats; women who are readier to fight than their husbands and women who scatter like leaves before the sound of a brawl; women whose hands are never still and women who sit after a heavy day's work with hands flaccid in their laps. And in front and beside and behind them, in their arms, on their backs, clinging with hands tight around their necks, sitting on their shoulders, slung in slings and net bags and baskets, hanging on the tipi wall tightly laced in cradle-boards--there are the children. Children who may be dressed as tiny replicas of the adults, tripping over a long skirt as they learn to walk, and children who may go stark-naked until ten or eleven; children who relax in passivity as their carriers pound rice or play rough running-games and children who brace themselves tightly as the punted canoe jams and mother and child fall into the water; children for whom there is no word, who are first referred to as "mice" or "beetles" and then as "small men," children who have been wanted, children whose very words are treated as prophecy; children who are subhuman until they have teeth and children who are monsters if their teeth come in irregularly; children who have no toys and who cling sullenly to the legs of adults and children who are as gay and frolicsome as laughter itself. . . .

The reader's mind will be filled with images quite different from mine. The problem will be whether as I write from this background those who read can keep such words as "men," "women," and "children" opened-ended words that carry an echo, although not the precise detail, of these varieties of human behavior out of the knowledge of which I am attempting to communicate.

A CROSS-CULTURAL SURVEY OF SOME SEX DIFFERENCES
IN SOCIALIZATION¹

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In our society, certain differences may be observed between the typical personality characteristics of the two sexes. These sex differences in personality are generally believed to result in part from differences in the way boys and girls are reared. To the extent that personality differences between the sexes are thus of cultural rather than biological origin, they seem potentially susceptible to change. But how readily susceptible to change? In the differential rearing of the sexes does our society make an arbitrary imposition on an infinitely plastic biological base, or is this cultural imposition found uniformly in all societies as an adjustment to the real biological differences between the sexes? This paper reports one attempt to deal with this problem.

Data and Procedures

The data used were ethnographic reports, available in the anthropological literature, about socialization practices of various cultures. One hundred and ten cultures, mostly nonliterate, were studied. They were selected primarily in terms of the existence of adequate ethnographic reports of socialization practices and secondarily so as to obtain a wide and reasonably balanced geographical distribution. Various aspects of socialization of infants and children were rated on a seven-point scale by two judges (Mrs. Bacon and Mr. Barry). Where the ethnographic reports permitted, separate ratings were made for the socialization of boys and girls. Each rating was indicated as either confident or doubtful; with still greater uncertainty, or with complete lack of evidence, the particular rating was, of course, not made at all. We shall restrict the report of sex difference ratings to cases in which both judges made a confident rating. Also omitted is the one instance where the two judges reported a sex difference in opposite directions, as it demonstrates only unreliability of judgment. The number of cultures that meet these criteria is much smaller than the total of 110; for the several variables to be considered, the number varies from 31 to 84.

The aspects of socialization on which ratings were made included:

1. Several criteria of attention and indulgence toward infants.
2. Strength of socialization from age four or five years until shortly before puberty, with respect to five systems of behavior; strength of socialization was defined as the combination of positive pressure (rewards for the behavior) plus negative pressure (punishments for lack of the behavior). The variables were:
 - (a) Responsibility or dutifulness training. (The data were such that training in the performance of chores in the productive or domestic economy was necessarily the principal source of information here; however, training in the performance of other duties was also taken into account when information was available.)
 - (b) Nurturance training, i.e., training the child to be nurturant or helpful toward younger siblings and other dependent people.
 - (c) Obedience training.
 - (d) Self-reliance training.
 - (e) Achievement training, i.e., training the child to orient his behavior toward standards of excellence in performance, and to seek to achieve as excellent a performance as possible.

Where the term "no sex difference" is used here, it may mean any of three things: (a) the judge found separate evidence about the training of boys and girls on this particular variable, and judged it to be identical; (b) the judge found a difference between the training of boys and girls, but not great enough for the sexes to be rated a whole point apart on a seven-point scale; (c) the judge found evidence only about the training of "children" on this variable, the ethnographer not reporting separately about boys and girls.

Sex Differences in Socialization

On the various aspects of attention and indulgence toward infants, the judges almost always agreed in finding no sex difference. Out of 96 cultures for which the ratings included the infancy period, 88 (92%) were rated with no sex difference by either judge for any of those variables. This result is consistent with the point sometimes made by anthropologists that "baby" generally is a single status undifferentiated by sex, even though "boy" and "girl" are distinct statuses.

On the variables of childhood socialization, on the other hand, a rating of no sex difference by both judges was much less common. This finding of no sex difference varied in frequency from 10% of the cultures for the obedience variable, as shown in the last column of Table 1. Where a sex difference is reported, by either one or both judges, the difference tends strongly to be in a particular direction, as shown in the earlier columns of the same table. Pressure toward nurturance, obedience, and responsibility is most often stronger for girls, whereas pressure toward achievement and self-reliance is most often stronger for boys.

For nurturance and self-reliance, all the sex differences are in the same direction. For achievement there is only one exception to the usual direction of difference, and for obedience only two; but for responsibility there are nine. What do these exceptions mean? We have re-examined all these cases. In most of them, only one judge had rated the sexes as differently treated (sometimes one judge, sometimes the other), and in the majority of these cases both judges were now inclined to agree that there was no convincing evidence of a real difference. There were exceptions, however, especially in cases where a more formal or systematic training of

Table 1
Ratings of Cultures for Sex Differences on Five Variables
of Childhood Socialization Pressure

Variable	Number of Cultures	Both Judges Agree in Rating the Variable Higher In		One Judge Rates No Difference, One Rates the Variable Higher in		Percentage of Cultures with Evidence of Sex Difference in Direction of		
		Girls	Boys	Girls	Boys	Girls	Boys	Neither
Nurturance	33	17	0	10	0	82%	0%	18%
Obedience	69	6	0	18	2	35%	3%	62%
Responsibility	84	25	2	26	7	61%	11%	28%
Achievement	31	0	17	1	10	3%	87%	10%
Self-reliance	82	0	64	0	6	0%	85%	15%

boys seemed to imply greater pressure on them toward responsibility. The most convincing cases were the Masai and Swazi, where both judges had originally agreed in rating responsibility pressures greater in boys than in girls. In comparing the five aspects of socialization we may conclude that responsibility shows by far the strongest evidence of real variation in the direction of sex difference, and obedience much the most frequently shows evidence of no sex difference at all.

In subsequent discussion we shall be assuming that the obtained sex differences in the socialization ratings reflect true sex differences in the cultural practices. We should consider here two other possible sources of these rated differences.

1. The ethnographers could have been biased in favor of seeing the same pattern of sex differences as in our culture. However, most anthropologists readily perceive and eagerly report novel and startling cultural features, so we may expect them to have reported unusual sex differences where they existed. The distinction between matrilineal and patrilineal,

and between matrilineal and patrilineal cultures, given prominence in many ethnographic reports, shows an awareness of possible variations in the significance of sex differences from culture to culture.

2. The two judges could have expected to find in other cultures the sex roles which are familiar in our culture and inferred them from the material on the cultures. However, we have reported only confident ratings, and such a bias seems less likely here than for doubtful ratings. It might be argued, moreover, that bias has more opportunity in the cases ambiguous enough so that only one judge reported a sex difference, and less opportunity in the cases where the evidence is so clear that both judges agree. Yet in general, as may be seen in Table 1, the deviant cases are somewhat more frequent among the cultures where only one judge reported a sex difference.

The observed differences in the socialization of boys and girls are consistent with certain universal tendencies in the differentiation of adult sex roles. In the economic sphere, men are more frequently allotted tasks that involve leaving home and engaging in activities where a high level of skill yields important returns; hunting is a prime example. Emphasis on training in self-reliance and achievement for boys would function as preparation for such an economic role. Women, on the other hand, are more frequently allotted tasks at or near home that minister most immediately to the needs of others (such as cooking and water carrying); these activities have a nurturant character, and in their pursuit a responsible carrying out of established routines is likely to be more important than the development of an especially high order of skill. Thus training in nurturance, responsibility, and, less clearly, obedience, may contribute to preparation for this economic role. These consistencies with adult role go beyond the economic sphere, of course. Participation in warfare, as a male prerogative, calls for self-reliance and a high order of skill where survival or death is the immediate issue. The childbearing which is biologically assigned to women, and the child care which is socially assigned primarily to them, lead to nurturant behavior and often call for a more continuous responsibility than do the tasks carried out by men. Most of these distinctions in adult role are not inevitable, but the biological differences between the sexes strongly predispose the distinction of role, if made, to be in a uniform direction. (2)

The relevant biological sex differences are conspicuous in adulthood but generally not in childhood. If each generation were left entirely to its own devices, therefore, without even an older generation to copy, sex differences in role would presumably be almost absent in childhood and would have to be developed after puberty at the expense of considerable relearning on the part of one or both sexes. Hence, a pattern of child training which foreshadows adult differences can serve the useful function of minimizing what Benedict termed "discontinuities in cultural conditioning."

The differences in socialization between the sexes in our society, then, are no arbitrary custom of our society, but a very widespread adaptation of culture to be biological substratum of human life.

Variation in Degree of Sex Differentiation

While demonstrating near-universal tendencies in direction of difference between the socialization of boys and girls, our data do not show perfect uniformity. A study of the variations in our data may allow us to see some of the conditions which are associated with, and perhaps, give rise to, a greater or smaller degree of this difference by two different methods, one more inclusive and the other more selective. In both methods the ratings were, at first, considered separately for each of the five variables. A sex-difference rating was made only if both judges made a rating on this variable and at least one judge's rating was confident.

In the more inclusive method the ratings were dichotomized, separately for each variable, as close as possible to the median into those showing a large and those showing a small sex difference. Thus, for each society a large or small sex difference was recorded for each of the five variables on which a sex-difference rating was available. A society was given an overall classification of large or small sex difference if it had a sex-difference rating on at least three variables and if a majority of these ratings agreed in being large, or agreed in being small. This method permitted classification of a large number of cultures, but the grounds for classification were capricious in many cases, as a difference of only one point in the rating of a single variable might change the overall classification of sex difference for a culture from large to small.

In the more selective method, we again began by dichotomizing each variable as close as possible to the median; but a society was now classified as having a large or small sex difference on the variable only if it was at least one step away from the scores immediately adjacent to the median. Thus only the more decisive ratings of sex difference were used. A culture was classified as having an overall large or small sex difference only if it was given a sex-difference rating which met this criterion on at least two variables, and only if all such ratings agreed in being large, or agreed in being small.

We then tested the relation of each of these dichotomies to 24 aspects of culture on which Murdock has categorized the customs of most of these societies (3) and which seemed of possible significance for sex differentiation. The aspects of culture covered include type of economy, residence pattern, marriage and incest rules, political integration, and social organization. For each aspect of culture, we grouped Murdock's categories to make a dichotomous contrast (sometimes omitting certain categories as irrelevant to the contrast). In the case of some aspects of culture, two or more separate contrasts were made (e.g., under form of marriage we contrasted monogamy with polygyny, and also contrasted sororal with nonsororal polygyny). For each of 40 comparisons thus formed, we prepared a 2 x 2 frequency table to determine relation to each of our sex-difference dichotomies. A significant relation was found for six of these 40 aspects of culture with the more selective dichotomization of overall sex difference. In four of these comparisons, the relation to the more inclusive dichotomization was also significant. These relationships are

all given in Table 2, in the form of phi coefficients, along with the outcome of testing significance by the use of χ^2 "squared" or Fisher's exact test. In trying to interpret these findings, we have also considered the nonsignificant correlations with other variables, looking for consistency and inconsistency with the general implications of the significant findings. We have arrived at the following formulation of results:

1. Large sex difference in socialization is associated with an economy that places a high premium on the superior strength, and superior development of motor skills requiring strength, which characterize the male. Four of the correlations reported in Table 2 clearly point to this generalization: the correlations of large sex difference with the hunting of large animals, with grain rather than root crops, with the keeping of large rather than small domestic animals, and with nomadic rather than sedentary residence. The correlation with the unimportance of fishing may also be consistent with this generalization, but the argument is not clear. (4)

Table 2
Culture Variables Correlated with Large
Sex Difference in Socialization,
Separately for Two Types
of Sample

Variable	More Selec- tive Sample		More Inclu- sive Sample	
	ϕ	N	ϕ	N
Large animals are hunted	.48*	(34)	.28*	(72)
Grain rather than root crops are grown	.82**	(20)	.62**	(43)
Large or milking animals rather than small animals are kept	.65*	(19)	.43*	(35)
Fishing unimportant or absent	.42*	(31)	.19	(69)
Nomadic rather than seden- tary residence	.61**	(34)	.15	(71)
Polygyny rather than monog- amy	.51*	(28)	.38**	(64)

* $p < .05$.

** $p < .01$.

Note--The variables have been so phrased that all correlations are positive. The phi coefficient is shown, and in parentheses, the number of cases on which the comparison was based. Significance level was determined by χ^2 "squared" or Fisher's exact test where applicable, using in all cases a two-tailed test.

Other correlations consistent with the generalization, though not statistically significant, are with hunting of large game rather than small game, and with the general importance of all hunting and gathering.

2. Large sex difference in socialization appears to be correlated with customs that make for a large family group with high cooperative interaction. The only statistically significant correlation relevant here is that with polygyny rather than monogamy. This generalization is, however, supported by several substantial correlations that fall only a little short of being statistically significant. One of these is a correlation with sororal rather than nonsororal polygyny: Murdock and Whiting have presented indirect evidence that co-wives generally show smoother cooperative interaction if they are sisters. Correlations are also found with the presence of either an extended or a polygynous family rather than the nuclear family only; with the presence of an extended family; and with the extreme contrast between maximal extension and no extension of the family. The generalization is also to some extent supported by small correlations with side extension of incest taboos, if we may presume that an incest taboo makes for effective unthreatening cooperation within the extended family. The only possible exception to this generalization, among substantial correlations, is a near-significant correlation with an extended or polygynous family's occupying a cluster of dwellings rather than a single dwelling. (5)

In seeking to understand this second generalization, we feel that the degree of social isolation of the nuclear family may perhaps be the crucial underlying variable. To the extent that the nuclear family must stand alone, the man must be prepared to take the woman's role when she is absent or incapacitated, and vice versa. Thus the sex differentiation cannot afford to be too great. But to the extent that the nuclear family is steadily interdependent with other nuclear families, the female role in the household economy can be temporarily taken over by another woman, or the male role by another man, so that sharp differentiation of sex role is no handicap.

The first generalization, which concerns the economy, cannot be viewed as dealing with material completely independent of the ratings of socialization. The training of children in their economic role was often an important part of the data used in rating socialization variables, and would naturally vary according to the general economy of the society. We would stress, however, that we were by no means using the identical data on the two sides of our comparison; we were on the one hand judging data on the socialization of children and on the other hand using Murdock's judgments on the economy of the adult culture. In the case of the second generalization, it seems to us that there was little opportunity for information on family and social structure to have influenced the judges in making the socialization ratings.

Both of these generalizations contribute to understanding the social background of the relatively small difference in socialization of boys and girls which we believe characterizes our society at the present time. Our mechanized economy is perhaps less dependent than any previous economy

upon the superior average strength of the male. The nuclear family in our society is often so isolated that husband and wife must each be prepared at times to take over or help in the household tasks normally assigned to the other. It is also significant that the conditions favoring low sex differentiation appear to be more characteristic of the upper segments of our society, in socioeconomic and educational status, than of lower segments. This observation may be relevant to the tendency toward smaller sex differences in personality in higher status groups (cf. Terman and Miles).

The increase in our society of conditions favoring small sex difference has led some people to advocate a virtual elimination of sex differences in socialization. This course seems likely to be dysfunctional even in our society. Parsons, Bales, et al. argue that a differentiation of role similar to the universal pattern of sex difference is an important and perhaps inevitable development in any social group, such as the nuclear family. If we add to their argument the point that biological differences between the sexes make most appropriate the usual division of those roles between the sexes, we have compelling reasons to expect that the decrease in differentiation of adult sex role will not continue to the vanishing point. In our training of children, there may now be less differentiation in sex role than characterizes adult life--so little, indeed, as to provide inadequate preparation for adulthood. This state of affairs is likely to be especially true of formal education, which is more subject to conscious influence by an ideology than is informal socialization at home. With child training being more oriented toward the male than the female role in adulthood, many of the adjustment problems of women in our society today may be partly traced to conflicts growing out of inadequate childhood preparation for their adult role. This argument is nicely supported in extreme form by Spiro's analysis of sex roles in an Israeli kibbutz. The ideology of the founders of the kibbutz included the objective of greatly reducing differences in sex roles. But the economy of the kibbutz is a largely nonmechanized one in which the superior average strength of men is badly needed in many jobs. The result is that, despite the ideology and many attempts to implement it, women continue to be assigned primarily to traditional "women's work," and the incompatibility between upbringing or ideology and adult role is an important source of conflict for women.

Note on regional distribution. There is marked variation among regions of the world in typical size of sex difference in socialization. In our sample, societies in North America and Africa tend to have large sex difference, and societies in Oceania to have small sex difference. Less confidently, because of the smaller number of cases, we can report a tendency toward small sex differences in Asia and South America as well. Since most of the variables with which we find the sex difference to be significantly correlated have a similar regional distribution, the question arises whether the correlations might better be ascribed to some quite different source having to do with large regional similarities, rather than to the functional dependence we have suggested. As a partial check, we have tried to determine whether the correlations we report in Table 2 tend also to be found strictly within regions. For each of the three regions for which we have sizable samples (North America, Africa, and Oceania) we have

separately plotted 2 x 2 tables corresponding to each of the six relationships reported in Table 2. (We did this only for the more inclusive sample, since for the more selective sample the number of cases within a region would have been extremely small.) Out of the eighteen correlations thus determined, eleven are positive and only three are negative (the other four being exactly zero). This result clearly suggests a general tendency for these correlations to hold true within regions as well as between regions, and may lend further support to our functional interpretation.

Summary

A survey of certain aspects of socialization in 110 cultures shows that differentiation of the sexes is unimportant in infancy, but that in childhood there is, as in our society, a widespread pattern of greater pressure toward nurturance, obedience, and responsibility in girls, and toward self-reliance and achievement-striving in boys. There are a few reversals of sex difference, and many instances of no detectable sex difference; these facts tend to confirm the cultural rather than directly biological nature of the differences. Cultures vary in the degree to which these differentiations are made; correlational analysis suggests some of the social conditions influencing these variations, and helps in understanding why our society has relatively small sex differentiation.

Footnotes

¹ This research is part of a project for which financial support was provided by the Social Science Research Council and the Ford Foundation. We are greatly indebted to G.P. Murdock for supplying us with certain data, as indicated below, and to him and Thomas W. Maretzki for suggestions that have been used in this paper.

² For data and interpretations supporting various arguments of this paragraph, see references cited for Mead, Murdock, and Scheinfeld.

³ These data were supplied to us directly by Professor Murdock.

⁴ Looking (with the more inclusive sample) into the possibility that this correlation might result from the correlation between fishing and sedentary residence, a complicated interaction between these variables was found. The correlation of sex differentiation with absence of fishing is found only in nomadic societies, where fishing is likely to involve cooperative activity of the two sexes, and its absence is likely to mean dependence upon the male for large game hunting or herding large animals (whereas in sedentary societies the alternatives to fishing do not so uniformly require special emphasis on male strength). The correlation of sex differentiation with nomadism is found only in nonfishing societies; here nomadism is likely to imply large game hunting or herding large animals, whereas in fishing societies nomadism evidently implies no such special dependence upon male strength. Maximum sex differentiation is found in nomadic nonfishing societies (fifteen with large difference and only two with small) and minimum sex differentiation in nomadic fishing societies (two with large difference and

seven with small difference). These findings further strengthen the argument for a conspicuous influence of the economy upon sex differentiation.

⁵ We think the reverse of this correlation would be more consistent with our generalization here. But perhaps it may reasonably be argued that the various nuclear families composing an extended or polygynous family are less likely to develop antagonisms which hinder cooperation if they are able to maintain some physical separation. On the other hand, this variable may be more relevant to the first generalization than to the second. Occupation of a cluster of dwellings is highly correlated with presence of herding and with herding of large rather than small animals, and these economic variables in turn are correlated with large sex difference in socialization. Occupation of a cluster of dwellings is also correlated with polygyny rather than monogamy and shows no correlation with sororal vs. nonsororal polygyny.

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Societal Bias in Science

The problem of bias in science is discussed further in the following article. Historically the status of women in primitive society has been misunderstood and underestimated because the interpretations made by anthropologists were based on culturally biased data. Sally Slocum finds it necessary to examine previously proposed theories for their male bias.

When you have finished reading this article, you may find it instructive to examine the data and conclusions presented in the preceding articles. Distinguish between fact and interpretation, evaluate the authors' conclusions for cultural bias, and suggest alternative conclusions.

WOMAN THE GATHERER:

MALE BIAS IN ANTHROPOLOGY*

by Sally Slocum

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Little systematic attention has been given in our discipline to an "anthropology of knowledge." While some anthropologists have concerned themselves with knowledge in general, as seen through the varieties of human cultures, few have examined anthropological knowledge itself. An anthropology of knowledge would have several parts. First is what Peter Berger (1967:1-18) has called "philosophical anthropology": a study of the nature of the human species. This has always been a legitimate concern of anthropology, but too often we become so concerned with minute differences that we forget we are studying a single species. Second is how we "know"

* This paper originally appeared under the name Sally Linton in the first version of Women in Perspective: A Guide for Cross-Cultural Studies (Urbana: University of Illinois Press, 1971), edited by Sue-Ellen Jacobs, to whom thanks are due for facilitating its reprinting (in Toward an Anthropology of Women).

Many of the ideas were developed during conversations with Jane Kephart and Joan Roos over a period of several months. Since it was the interaction which produced the ideas, it is difficult to credit any particular idea to any one person. Suffice it to say that without their help and encouragement this paper would not have been written. *

anything--what is accepted as "proof," what is reality, what are the grounds for rationality (Garfinkel, 1960), what modes are used in gathering knowledge, what are the effects of differences in culture and world view on what we "know." Third is a close examination of the questions asked in anthropology, for questions always determine and limit answers.

It is the third point, the nature of anthropological questions, to which I wish to speak in this paper. We are human beings studying other human beings, and we cannot leave ourselves out of the equation. We choose to ask certain questions, and not others. Our choice grows out of the cultural context in which anthropology and anthropologists exist. Anthropology, as an academic discipline, has been developed primarily by white Western males, during a specific period in history. Our questions are shaped by the particulars of our historical situation, and by unconscious cultural assumptions.

Given the cultural and ethnic background of the majority of anthropologists, it is not surprising that the discipline has been biased. There are signs, however, that this selective blindness is beginning to come under scrutiny. For example, in the exchange in the journal Current Anthropology (1968), anthropologists like Kathleen Gough and Gerald Berreman point out the unconscious effects of American political and economic assumptions on our selection of problems and populations to be studied. Restive minority groups in this country are pointing to the bias inherent in anthropological studies of themselves through books such as Vine Deloria's Custer Died for Your Sins. We have always encouraged members of American minority groups, and other "foreigners," to take up anthropology because of the perspective on the world that they can supply. The invitation is increasingly being accepted. As we had both hoped and feared, repercussions from this new participation are being felt in theory, method, interpretation, and problem choice, shaking anthropology to the roots.

The perspective of women is, in many ways, equally foreign to an anthropology that has been developed and pursued primarily by males. There is a strong male bias in the questions asked, and the interpretations given. This bias has hindered the full development of our discipline as "the study of the human animal" (I don't want to call it "the study of man" for reasons that will become evident). I am going to demonstrate the Western male bias by re-examining the matter of evolution of *Homo sapiens* from our nonhuman primate ancestors. In particular, the concept of "Man the Hunter" as developed by Sherwood Washburn and C. Lancaster (1968) and others is my focus. This critique is offered in hopes of transcending the male bias that limits our knowledge by limiting the questions we ask.

Though male bias could be shown in other areas, hominid evolution is particularly convenient for my purpose because it involves speculations and inferences from a rather small amount of data. In such a case, hidden assumptions and premises that lie behind the speculations and inferences are more easily demonstrated. Male bias exists not only in the ways in which the scanty data are interpreted, but in the very language used. All too often the word "man" is used in such an ambiguous fashion that it is

impossible to decide whether it refers to males or to the human species in general, including both males and females. In fact, one frequently is led to suspect that in the minds of many anthropologists, "man," supposedly meaning the human species, is actually exactly synonymous with "males."

This ambiguous use of language is particularly evident in the writing that surrounds the concept of "Man the Hunter." Washburn and Lancaster make it clear that it is specifically males who hunt, that hunting is much more than simply an economic activity, and that most of the characteristics which we think of as specifically human can be causally related to hunting. They tell us that hunting is a whole pattern of activity and way of life: "The biology, psychology, and customs that separate us from the apes--all these we owe to the hunters of time past." (1968:303) If this line of reasoning is followed to its logical conclusion, one must agree with Jane Kephart when she says:

Since only males hunt, and the psychology of the species was set by hunting, we are forced to conclude that females are scarcely human, that is, do not have built-in the basic psychology of the species: to kill and hunt and ultimately to kill others of the same species. The argument implies built-in aggression in human males, as well as the assumed passivity of human females and their exclusion from the mainstream of human development. (1970:5)

To support their argument that hunting is important to human males, Washburn and Lancaster point to the fact that many modern males still hunt, though it is no longer economically necessary. I could point out that many modern males play golf, play the violin, or tend gardens: these, as well as hunting, are things their culture teaches them. Using a "survival" as evidence to demonstrate an important fact of cultural evolution can be accorded no more validity when proposed by a modern anthropologist than when proposed by Tylor.

Regardless of its status as a survival, hunting, by implication as well as direct statement, is pictured as a male activity to the exclusion of females. This activity, on which we are told depends the psychology, biology, and customs of our species, is strictly male. A theory that leaves out half the human species is unbalanced. The theory of "Man the Hunter" is not only unbalanced; it leads to the conclusion that the basic human adaptation was the desire of males to hunt and kill. This not only gives too much importance to aggression, which is, after all, only one factor of human life, but it derives culture from killing. I am going to suggest a less biased reading of the evidence, which gives a more valid and logical picture of human evolution, and at the same time a more hopeful one. First I will note the evidence, discuss the more traditional reading of it, and then offer an alternative reconstruction.

The data we have to work from are a combination of fossil and archeological materials, knowledge of living nonhuman primates, and knowledge of living humans. Since we assume that the protohominid ancestors of *Homo sapiens* developed in a continuous fashion from a base of characteristics

similar to those of living nonhuman primates, the most important facts seem to be the ways in which humans differ from nonhuman primates, and the ways in which we are similar. The differences are as follows: longer gestation period; more difficult birth; neoteny, in that human infants are less well developed at birth; long period of infant dependency; absence of body hair; year-round sexual receptivity of females, resulting in the possibility of bearing a second infant while the first is still at the breast or still dependent; erect bipedalism; possession of a large and complex brain that makes possible the creation of elaborate symbolic systems, languages, and cultures, and also results in most behavior being under cortical control; food sharing; and finally, living in families. (For the purposes of this paper I define families as follows: a situation where each individual has defined responsibilities and obligations to a specific set of others of both sexes and various ages. I use this definition because, among humans, the family is a social unit, regardless of any biological or genetic relationship which may or may not exist among its members.)

In addition to the many well-known close physiological resemblances, we share with nonhuman primates the following characteristics: living in social groups; close mother-infant bonds; affectional relationships; a large capacity for learning and a related paucity of innate behaviors; ability to take part in dominance hierarchies; a rather complex nonsymbolic communication system which can handle with considerable subtlety such information as the mood and emotional state of the individual, and the attitude and status of each individual toward the other members of the social group.

The fossil and archeological evidence consists of various bones labeled *Ramapithecus*, *Australopithecus*, *Homo habilis*, *Homo erectus*, etc.; and artifacts such as stone tools representing various cultural traditions, evidence of use of fire, etc. From this evidence we can make reasonable inferences about diet, posture and locomotion, and changes in the brain as shown by increased cranial capacity, ability to make tools, and other evidences of cultural creation. Since we assume that complexity of material culture requires language, we infer the beginnings of language somewhere between *Australopithecus* and *Homo erectus*.

Given [these] data, the speculative reconstruction begins. As I was taught anthropology, the story goes something like this. Obscure selection pressures pushed the protohominid in the direction of erect bipedalism--perhaps the advantages of freeing the hands for food carrying or for tool use. Freeing the hands allowed more manipulation of the environment in the direction of tools for gathering and hunting food. Through a hand-eye-brain feedback process, coordination, efficiency, and skill were increased. The new behavior was adaptive, and selection pressure pushed the protohominid further along the same lines of development. Diet changed as the increase in skill allowed the addition of more animal protein. Larger brains were selected for, making possible transmission of information concerning tool making, and organizing cooperative hunting. It is assumed that as increased brain size was selected for, so also was neoteny--immaturity of infants at birth with a corresponding increase in their period of dependency, allowing more time

for learning at the same time as this learning became necessary through the further reduction of instinctual behaviors and their replacement by symbolically invented ones.

Here is where one may discover a large logical gap. From the difficult-to-explain beginning trends toward neoteny and increased brain size, the story jumps to "Man the Hunter." The statement is made that the females were more burdened with dependent infants and could not follow the rigorous hunt. Therefore they stayed at a "home base," gathering what food they could, while the males developed cooperative hunting techniques, increased their communicative and organizational skills through hunting, and brought the meat back to the dependent females and young. Incest prohibitions, marriage, and the family (so the story goes) grew out of the need to eliminate competition between males for females. A pattern developed of a male hunter becoming the main support of "his" dependent females and young (in other words, the development of the nuclear family for no apparent reason). Thus the peculiarly human social and emotional bonds can be traced to the hunter bringing back the food to share. Hunting, according to Washburn and Lancaster, involved "cooperation among males, planning, knowledge of many species and large areas, and technical skill." (1968:296) They even profess to discover the beginnings of art in the weapons of the hunter. They point out that the symmetrical Acheulian biface tools are the earliest beautiful man-made objects. Though we don't know what these tools were used for, they argue somewhat tautologically that the symmetry indicates they may have been swung, because symmetry only makes a difference when irregularities might lead to deviations in the line of flight. "It may well be that it was the attempt to produce efficient high-speed weapons that first produced beautiful, symmetrical objects." (1968:298)

So, while the males were out hunting, developing all their skills, learning to cooperate, inventing language, inventing art, creating tools and weapons, the poor dependent females were sitting back at the home base having one child after another (many of them dying in the process), and waiting for the males to bring home the bacon. While this reconstruction is certainly ingenious, it gives one the decided impression that only half the species--the male half--did any evolving. In addition to containing a number of logical gaps, the argument becomes somewhat doubtful in the light of modern knowledge of genetics and primate behavior.

The skills usually spoken of as being necessary to, or developed through, hunting are things like coordination, endurance, good vision, and the ability to plan, communicate, and cooperate. I have heard of no evidence to indicate that these skills are either carried on the Y chromosome, or are triggered into existence by the influence of the Y chromosome. In fact, on just about any test we can design (psychological, aptitude, intelligence, etc.) males and females score just about the same. The variation is on an individual, not a sex, basis.

Every human individual gets half its genes from a male and half from a female; genes sort randomly. It is possible for a female to end up with all her genes from male ancestors, and for a male to end up with all his

genes from female ancestors. The logic of the hunting argument would have us believe that all the selection pressure was on the males, leaving the females simply as drags on the species. The rapid increase in brain size and complexity was thus due entirely to half the species; the main function of the female half was to suffer and die in the attempt to give birth to their large-brained male infants. An unbiased reading of the evidence indicates there was selection pressure on both sexes, and that hunting was not in fact the basic adaptation of the species from which flowed all the traits we think of as specifically human. Hunting does not deserve the primary place it has been given in the reconstruction of human evolution, as I will demonstrate by offering the following alternate version.

Picture the primate band: each individual gathers its own food, and the major enduring relationship is the mother-infant bond. It is in similar circumstances that we imagine the evolving protohominids. We don't know what started them in the direction of neoteny and increased brain size, but once begun the trends would prove adaptive. To explain the shift from the primate individual gathering to human food sharing, we cannot simply jump to hunting. Hunting cannot explain its own origin. It is much more logical to assume that as the period of infant dependency began to lengthen, the mothers would begin to increase the scope of their gathering to provide food for their still-dependent infants. The already strong primate mother-infant bond would begin to extend over a longer time period, increasing the depth and scope of social relationships, and giving rise to the first sharing of food.

It is an example of male bias to picture these females with young as totally or even mainly dependent on males for food. Among modern hunter-gatherers, even in the marginal environments where most live, the females can usually gather enough to support themselves and their families. In these groups gathering provides the major portion of the diet, and there is no reason to assume that this was not also the case in the Pliocene or early Pleistocene. In the modern groups women and children both gather and hunt small animals, though they usually do not go on the longer hunts. So, we can assume a group of evolving protohominids, gathering and perhaps beginning to hunt small animals, with the mothers gathering quite efficiently both for themselves and their offspring.

It is equally biased, and quite unreasonable, to assume an early or rapid development of a pattern in which one male was responsible for "his" female(s) and young. In most primate groups when a female comes into estrus she initiates coitus or signals her readiness by presenting. The idea that a male would have much voice in "choosing" a female, or maintain any sort of individual, long-term control over her or her offspring, is surely a modern invention which could have had no place in early hominid life. (Sexual control over females through rape or the threat of rape seems to be a modern invention. Primate females are not raped because they are willing throughout estrus, and primate males appear not to attempt coitus at other times, regardless of physiological ability.) In fact, there seems to be no reason for suggesting the development of male-female

adult pair-bonding until much later. Long-term monogamy is a fairly rare pattern even among modern humans--I think it is a peculiarly Western male bias to suppose its existence in protohuman society. An argument has been made (by Morris, 1967, and others) that traces the development of male-female pair-bonding to the shift of sexual characteristics to the front of the body, the importance of the face in communication, and the development of face-to-face coitus. This argument is insufficient in the first place because of the assumption that face-to-face coitus is the "normal," "natural," or even the most common position among humans (historical evidence casts grave doubt on this assumption). It is much more probable that the coitus position was invented after pair-bonding had developed for other reasons.

Rather than adult male-female sexual pairs, a temporary consort-type relationship is much more logical in hominid evolution. It is even a more accurate description of the modern human pattern: the most dominant males (chief, headman, brave warrior, good hunter, etc.) mate with the most dominant females (in estrus, young and beautiful, fertile, rich, etc.) for varying periods of time. Changing sexual partners is frequent and common. We have no way of knowing when females began to be fertile year-round, but this change is not a necessary condition for the development of families. We need not bring in any notion of paternity, or the development of male-female pairs, or any sort of marriage in order to account for either families or food sharing.

The lengthening period of infant dependency would have strengthened and deepened the mother-infant bond; the earliest families would have consisted of females and their children. In such groups, over time, the sibling bond would have increased in importance also. The most universal, and presumably oldest, form of incest prohibition is between mother and son. There are indications of such avoidance even among modern monkeys. It could develop logically from the mother-children family: as the period of infant dependency lengthened, and the age of sexual maturity advanced, a mother might no longer be capable of childbearing when her son reached maturity. Another factor which may have operated is the situation found in many primates today where only the most dominant males have access to fertile females. Thus a young son, even after reaching sexual maturity, would still have to spend time working his way up the male hierarchy before gaining access to females. The length of time it would take him increases the possibility that his mother would no longer be fertile.

Food sharing and the family developed from the mother-infant bond. The techniques of hunting large animals were probably much later developments, after the mother-children family pattern was established. When hunting did begin, and the adult males brought back food to share, the most likely recipients would be first their mothers, and second their siblings. In other words, a hunter would share food not with a wife or sexual partner, but with those who shared food with him: his mother and siblings.

It is frequently suggested or implied that the first tools were, in fact, the weapons of the hunters. Modern humans have become so accustomed to the thought of tools and weapons that it is easy for us to imagine the first manlike creature who picked up a stone or club. However, since we don't really know what the early stone tools such as hand-axes were used for, it is equally probable that they were not weapons at all, but rather aids in gathering. We know that gathering was important long before much animal protein was added to the diet, and continued to be important. Bones, sticks, and hand-axes could be used for digging up tubers or roots, or to pulverize tough vegetable matter for easier eating. If, however, instead of thinking in terms of tools and weapons, we think in terms of cultural inventions, a new aspect is presented. I suggest that two of the earliest and most important cultural inventions were containers to hold the products of gathering, and some sort of sling or net to carry babies. The latter in particular must have been extremely important with the loss of body hair and the increasing immaturity of neonates, who could not cling and had less and less to cling to. Plenty of material was available--vines, hides, human hair. If the infant could be securely fastened to the mother's body, she could go about her tasks much more efficiently. Once a technique for carrying babies was developed, it could be extended to the idea of carrying food, and eventually to other sorts of cultural inventions--choppers and grinders for food preparation, and even weapons. Among modern hunter-gatherers, regardless of the poverty of their material culture, food carriers and baby carriers are always important items in their equipment.

A major point in the "Man the Hunter" argument is that cooperative hunting among males demanded more skill in social organization and communication, and thus provided selection pressure for increased brain size. I suggest that longer periods of infant dependency, more difficult births, and longer gestation periods also demanded more skills in social organization and communication--creating selective pressure for increased brain size without looking to hunting as an explanation. The need to organize for feeding after weaning, learning to handle the more complex social-emotional bonds that were developing, the new skills and cultural inventions surrounding more extensive gathering--all would demand larger brains. Too much attention has been given to the skills required by hunting, and too little to the skills required for gathering and the raising of dependent young. The techniques required for efficient gathering include location and identification of plant varieties, seasonal and geographical knowledge, containers for carrying the food, and tools for its preparation. Among modern hunting-gathering groups this knowledge is an extremely complex, well-developed, and important part of their cultural equipment. Caring for a curious, energetic, but still dependent human infant is difficult and demanding. Not only must the infant be watched, it must be taught the customs, dangers, and knowledge of its group. For the early hominids, as their cultural equipment and symbolic communication increased, the job of training the young would demand more skill. Selection pressure for better brains came from many directions.

Much has been made of the argument that cooperation among males demanded by hunting acted as a force to reduce competition for females.

I suggest that competition for females has been greatly exaggerated. It could easily have been handled in the usual way for primates--according to male status relationships already worked out--and need not be pictured as particularly violent or extreme. The seeds of male cooperation already exist in primates when they act to protect the band from predators. Such dangers may well have increased with a shift to savannah living, and the longer dependency of infants. If biological roots are sought to explain the greater aggressiveness of males, it would be more fruitful to look toward their function as protectors, rather than any supposedly basic hunting adaptation. The only division of labor that regularly exists in primate groups is the females caring for infants and the males protecting the group from predators. The possibilities for both cooperation and aggression in males lie in this protective function.

The emphasis on hunting as a prime moving factor in hominid evolution distorts the data. It is simply too big a jump to go from the primate individual gathering pattern to a hominid cooperative hunting-sharing pattern without some intervening changes. Cooperative hunting of big game animals could only have developed after the trends toward neoteny and increased brain size had begun. Big-game hunting becomes a more logical development when it is viewed as growing out of a complex of changes which included sharing the products of gathering among mothers and children, deepening social bonds over time, increase in brain size, and the beginnings of cultural invention for purposes such as baby carrying, food carrying, and food preparation. Such hunting not only needed the prior development of some skills in social organization and communication; it probably also had to await the development of the "home base." It is difficult to imagine that most or all of the adult primate males in a group would go off on a hunting expedition, leaving the females and young exposed to the danger of predators, without some way of communicating to arrange for their defense, or at least a way of saying, "Don't worry, we'll be back in two days." Until that degree of communicative skill developed, we must assume either that the whole band traveled and hunted together, or that the males simply did not go off on large cooperative hunts.

The development of cooperative hunting requires, as a prior condition, an increase in brain size. Once such a trend is established, hunting skills would take part in a feedback process of selection for better brains just as would other cultural inventions and developments such as gathering skills. By itself, hunting fails to explain any part of human evolution and fails to explain itself.

Anthropology has always rested on the assumption that the mark of our species is our ability to symbol, to bring into existence forms of behavior and interaction, and material tools with which to adjust and control the environment. To explain human nature as evolving from the desire of males to hunt and kill is to negate most of anthropology. Our species survived and adapted through the invention of culture, of which hunting is simply a part. It is often stated that hunting must be viewed as the "natural" species' adaptation because it lasted as long as it did, nine-tenths of all human history. However:

Man the Hunter lasted as long as "he" did from no natural propensity toward hunting any more than toward computer programming or violin playing or nuclear warfare, but because that was what the historical circumstances allowed. We ignore the first premise of our science if we fail to admit that "man" is no more natural a hunter than "he" is naturally a golfer, for after symboling became possible our species left forever the ecological niche of the necessity of any one adaptation, and made all adaptations possible for ourselves. (Kephart, 1970:23)

That the concept of "Man the Hunter" influenced anthropology for as long as it did is a reflection of male bias in the discipline. This bias can be seen in the tendency to equate "man," "human," and "male"; to look at culture almost entirely from a male point of view; to search for examples of the behavior of males and assume that this is sufficient for explanation, ignoring almost totally the female half of the species; and to filter this male bias through the "ideal" modern Western pattern of one male supporting a dependent wife and minor children.

The basis of any discipline is not the answers it gets, but the questions it asks. As an exercise in the anthropology of knowledge, this paper stems from asking a simple question: what were the females doing while the males were out hunting? It was only possible for me to ask this question after I had become politically conscious of myself as a woman. Such is the prestige of males in our society that a woman, in anthropology or any other profession, can only gain respect or be attended to if she deals with questions deemed important by men. Though there have been women anthropologists for years, it is rare to be able to discern any difference between their work and that of male anthropologists. Learning to be an anthropologist has involved learning to think from a male perspective, so it should not be surprising that women have asked the same kinds of questions as men. But political consciousness, whether among women, blacks, American Indians, or any other group, leads to re-examination and re-evaluation of taken-for-granted assumptions. It is a difficult process, challenging the conventional wisdom, and this paper is simply a beginning. The male bias in anthropology that I have illustrated here is just as real as the white bias, the middle-class bias, and the academic bias that exist in the discipline. It is our task, as anthropologists, to create a "study of the human species" in spite of, or perhaps because of, or maybe even by means of, our individual biases and unique perspectives.

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Language

In the previous articles we have illustrated the effect of bias on the types of research questions asked, on the interpretation of data, and on the generalizations conveyed from science to society. In addition to these consequences of socialization, more subtle biases pervade our mode of thinking. Science, as well as society, is shaped by ideas, and ideas, except for abstract mathematical symbolism, are shaped by words. It is impossible to escape the surrounding culture completely, and it is often difficult to recognize the subtle cultural biases expressed even when a scientist is trying to be objective. Examples are use of "men" for groups that include women and use of only the masculine pronoun to refer to a scientist or official who may be either a woman or a man. Look through this book or other readings for specific cases, e.g. Cole and Cole's reference to scientists in their article on social stratification, and DuBridge's comments on the Science Advisor in his discussion of science and politics.

Both men and women may be so enmeshed in cultural assumptions of lower female status that they unwittingly perpetuate such an attitude in their scientific work. These cultural influences are expressed in the choice of words used to describe women and their activities and in the language employed by women in talking about themselves. One aspect of the language problem, words used to describe women, is considered in the following article.

TALKING ABOUT WOMEN

by Mary Ritchie Key

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Gingerbread Men and Gingerbread Girls: Label and Descriptors

It is a commonplace that labels and words which are used to refer to people and to describe others are based on deep-seated attitudes toward those particular people or groups of persons. It is also true, however, that these basic and underlying attitudes are not often acknowledged, or even recognized. It is very difficult, for example, for anyone to explain satisfactorily the difference between "lady" and "woman." Yet these terms are used among the population in very orderly and systematic patterns that express attitudes toward females or toward a particular female. The difference, perhaps, can be illustrated by two expressions: "The women,

God help us!" and "The ladies, God bless them!" (1) "Lady" is deceptively used, at times, as a form of flattery, supposedly to exalt the person and lift her above the ordinary. Every woman has been told at some time in her life that she shouldn't want to be equal with men because she is already superior! In fact, the result is to put her aside on the pedestal and not have to deal with her. Business and government cannot be run by committees and executive positions staffed with pedestal-ladies. These jobs have to be done by people whose feet are on the ground.

The etymology, or the history of the meanings of a word, is sometimes, and wrongly, used to control the dimensions of conduct permitted persons referred to by that word. For example, the origin of "woman" goes back to "wifman" or "wife-man"--pointing toward the belief that a woman has no being apart from a husband. Sometimes we refer to the past to try to justify our own beliefs. Quite apart from being unfair, this use of the history of words is linguistically unsound. The word "silly," for example, used to have a meaning "blessed," and still does in some cases in German. Today, in English, however, it would be difficult to foist the concept of blessedness on a silly fool! Likewise, to try to bring the meanings of long-ago into the words, "woman" or "lady" or "mistress," and direct the status of females based upon those meanings, even though historically of interest, is nonsensical. Over-enthusiasts even create fictitious etymologies. It is said that Ruskin often reminded the married women in his audiences that their place was in the home because "wife" meant "she who weaves." (2) Not so. But even if it were, the original meaning of "lady" was "bread-kneader." Would Ruskin send the ladies back to kneading dough?

When the labels for male and female are paired, we see other social structures exemplified. The couplet "man and wife" instead of "husband and wife" suggests that her existence is in relationship to the man and implies a subordinate position, the "Other" which Simone de Beauvoir speaks of. (3) The words "man" and "woman" have their counterparts "master" and "mistress" but the respective meanings of these pairs are vastly different. A woman's master is nothing like a man's mistress! One can be effeminate, but one cannot be emasculate. We know what a ladies' man is, but who has heard of a gentleman's woman?

Some words have as yet no counterpart. The term "housewife" has stood alone for centuries with no "househusband." Lately I saw the term "househusband" in an article that had to do with shopping for nourishing food. This makes sense when one notes the growing number of male shoppers in the supermarket. The word "housewife" is often used with modifiers which must be considered in the total meaning of the term. Recently, in the letters to the editor column of a newspaper, a writer referred to herself as: "a young housewife," "a dumb housewife," and "just a housewife." If the English language begins to embrace the concept and term for "househusband," it is likely that these modifying elements will change to something like a hypothetical: "a vigorous househusband/wife," or "an intelligent househusband/wife," or "a distinguished househusband/wife." That kind of language change will come with social change.

The counterparts in couplets can also designate distinct status relationships. In the military, where status is a well-defined concept by rank, one refers to "officers and their ladies" and "enlisted men and their wives." Linguistic labels in the matter of marital status reflect the attitudes of society toward male and female. A previously married man is a "bachelor"; a previously married woman is a "divorcée," with the implications being that a bachelor has a future, and a divorcée has a past.

A baffling lack in the matter of labels is that there is no way for persons "over thirty" to refer to their constant companion of the opposite sex. "Boyfriend" or "girlfriend" sounds so teen-agish and "lover" doesn't seem to be a discreet way to talk about someone in a general context. Language again reflects the Puritanical tradition of obliterating any possible male/female relationship except man and wife. Another lack in English that is devastating and inhibiting is the deficiency in our language for warm and respectable terms to refer to the sex act. (4) How pathetic it is that the only way we can discuss this function is to use scientific Latin words or "obscenities." Other languages are richer in their paradigms for making love.

Adult females often are labeled "girl" and "baby" even though they may be perfectly responsible members of society. Only recently a newscaster announced that "a California girl" had assumed command of the WAVES. This officer had the rank of Commander, a very distinguished rank in the Navy, and was later promoted to Captain. The parallel construction is conspicuously absent: "A California boy just assumed the position of Secretary of HEW."

Labels for females range from these mild forms which reflect social structures to insulting terms which reflect individual attitudes. In a society which claims to have the highest regard for women in the Christian tradition, it is indeed shocking to discover that our state and government leaders can still use offensive vocabulary in reference to females in public offices. A generation ago an important dictionary maker noted that "Over the years many terms for the word woman have become degraded." (5) Apparently discrimination against women is still socially acceptable. Two illustrations will suffice. In a discussion of committees during the last national election, one of our U.S. senators prescribed that no women would be permitted on a certain committee. He simply declared, "No broads!" and would discuss the issue no more. This is analogous to using such words as "wop, greaser, hunky, coon, or nigger," but almost no public official would dare use those words these days. This acceptance of insulting language can go to the extreme. Recently the trash barrels at the California Institution for Men, a corrective place, were painted with the silhouette of a woman, with a girl's name, and the words, "Use me." One of the barrels had a silhouette that was obviously pregnant, with the accompanying words, "I've been used." A complaint was made to the official in charge that this was inappropriate in a public institution using tax funds, but he defended the practice as "simply a take-off on current advertising of a well-known airline." Only when extensive pressure was brought to bear were the barrels repainted.

It should be pointed out here that these examples are not drawn from a limited and special segment of the population. These were not high criminals negotiating in a back room, not trash containers in an off-beat bawdy house; rather these examples concern tax-supported public offices. The most notable thing about such linguistic behavior is how widely it is tolerated. Does this say something about masochistic/sadistic tendencies in human beings? Do human beings need to insult and to be insulted to survive?

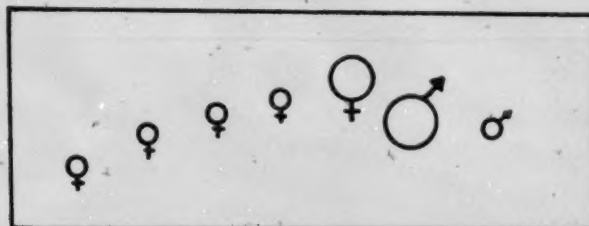
Bias in the language eventually gets caught in the trap of the ludicrous or the incongruous. A book advertisement said that Elizabeth I matured into a "true prince." Pearl Bailey and Hazel Kohring each received a "Man of the Year" award. A female opera singer who had a great voice was called a "Caruso in petticoats." And we learn that "menstrual pain accounts for an enormous loss of man-hours of work." The column "Men and Events" often has newsworthy items about females. One can be appalled at the unthinking, uncreative, and limited use of language in these instances.

A study of the descriptions of females who are in public or professional positions provides other examples of the actual values put on females in society. The following are illustrations I have collected from magazines and newspapers. The women described are not women who won the lottery, but are women who have positions entailing a good deal of training and expertise. The women were variously described as: "a serene, delicately formed woman" (referring to an executive chairperson); "a brown-eyed cutie" (referring to an athlete); "(she) speaks softly . . . blushes and laughs . . ." (referring to a commanding officer); "a very feminine woman" (referring to a chief of a Flight Service Station). A pilot was headlined as "Woman flier. . . ." A member of a commission was designated a "mother." A book which received considerable attention was written by a "housewife with an Oxford degree in English." Apparently an attempt is being made to assure women they can still remain sexually attractive even if professionally competent. I have not found similar examples for males of equal professional status. The focus on description for this caliber of occupation, one would expect, should be on qualifications for the job--not physical or emotional attributes. These illustrations indicate something of the immaturity of society in relating to females.

Careful observation of all kinds of written materials produces illustrations of prejudice which we all passed over unthinkingly until recently: "A cry or a scream of a monkey, like that produced by a frightened woman. . . ."; "A woman from the East Coast, nearly in tears. . . ."; "(the accent or dialect) of Kentucky and Tennessee . . . its feminine shrillness. . . ." The honored and prestigious Encyclopaedia Britannica speaks, in regard to Catherine of Aragon, of her "failure to produce a male to the throne. . . ." It wasn't Henry's syphilis, but Catherine's failure! In a discussion of the development of Pennsylvania, an historian says, "The new constitution. . . remedied all the injustices. . . providing for equality of representation . . . and for universal male suffrage. Pennsylvania had now become a genuine democracy. (6) An advertisement to attract young women to a training program promised the candidates that they would be

trained in "qualities every man seeks in a wife." Bets were taken on the Miss World contest in London's gambling circles, and the bookmakers admitted that the talk about the "favorites" did make the girls sound like racehorses.

The matter of order is of concern to scientists who study behavioral events. There are hierarchies and values seen in whether elements in the study come first or last, or which come before others. For example, in the wills filed in the vital statistics, it was common practice in the old days to list the children of the deceased with the sons listed before the daughters, if the daughters were named at all. This precedent of order preference still holds in the thinking of some people today. During the recent presidential campaign a popular magazine printed a picture of Eleanor and George McGovern with their five children. The seating position of the family itself indicates no special relationships. The caption under the



picture, however, lists the son first, and then returns to the left side of the picture and lists the daughters. This high value put on sons is, of course, of very long standing. Thomas Aquinas instructed his followers that in a fire, a man saves first his sons, then his wife.

The terminology which is used in labeling and describing members of society is a subtle (but sometimes not so subtle) reflection of the structures of society in terms of status, quality, permitted performance, and values. For hundreds and thousands of years human beings have used this kind of language mostly without ever recognizing its discriminatory impact and without challenging its continuance. Only recently, with the greater linguistic understanding of use of language in all situations have these biased elements come into focus in observing male and female linguistic behavior.

The Silent Woman: Tyranny in Language

"Silence gives the proper grace to women."

--Sophocles (7)

One might wonder why, in a book on language, there is a chapter on silence. In a linguistic sense, patterns of silence figure in language structures and must be dealt with in the analysis of language. Thus, on a micro-level, silence can be used to separate and give emphasis to different words. On another level, silence separates different speakers, except in simultaneous speech or in interrupting behavior. This kind of silence must be distinguished from nothingness, or moments when nothing is happening. Silence is part of language and is as important as sound to communication and thought. Silence means different things depending upon the users: master/slave, teenager/adult, male/female.

In still another capacity, silence has a global meaning among certain categories of human beings who illustrate an intellectual or creative silence. I have been haunted through the years by a question from one of my male students--a sensitive, bright young scholar, who asked the old question, "Why aren't there great women composers, artists, and writers?" The fact is, women have been "silent." We have to deal here with the paradox that women, with a reputation for volubility, have made all-too-few extra-biological contributions to society. I do not believe that the explanation for this phenomenon is strictly biological. There is a great deal of evidence that the expectations of society--presented to each of us at birth--are powerful enough by themselves, without biological imperatives, to thrust males into often inappropriate roles of dominance, and to similarly paralyze females into silence.

Ingmar Bergman created a complete film with the main character speechless throughout the movie. The protagonist in Persona was an actress who reached a crisis before the plot was presented to the audience. She became paralyzed--her vocal cords refused to play the game any longer. Her voice was stopped during a performance of Medea. One cannot forget that the ancient drama of Medea dealt with the role of motherhood. Bergman's actress was also a mother--a classical archetype of talent conflicting with duty. In the film the pleas and letters from home to her hospital bed and convalescent retirement were focused on their needs for her return to be wife and mother--not to return to her great acting accomplishments!

In any discussion of silence, it must be made clear that there are many varieties. (8) Silence is communicative and there is the comforting, companionable silence between people in equilibrium, which must be distinguished from the negative, disturbing silence of anger, fear, or hate. (9) Historically, women are expected not to make substantive verbal contributions in the mixed social setting. . . . A woman's role is to twitter about the weather, give opinions that are dull and charming and brief, and respond with "Yes?" and "Really?" during the men's dialogue. A male can use linguistic silence to produce intellectual silence in women. Randolph Churchill, Winston's father, had a maxim to handle women who wanted to

enter the discussions, "Gentlemen, there is only one reply to a lady when she argues with you--silence." (10)

Ben Jonson (ca. 1572-1637) wrote a satiric comedy called The Silent Woman. Elsewhere he noted the "Women are but Men's Shadows." (11) Chaucer had also observed, through the exceptional mouth of the Wife of Bath in Canterbury Tales, that women had not commented on men in anything like the manner that men had written of women. Indeed, as we noted before, a trip to any library and a browse through the stacks will show shelf after shelf of books about women--written by men. What do women have to say--about themselves . . . or the world around them? It is a commonplace that silence is necessary for deep thought--for either the artist or the scientist. The centuries of women's silence might be explained, in part, by the repression caused by not having private periods of productive silence. Lack of silence, in this respect, can produce silence. (12)

Mourning is a specific situation where women were assigned to absolute verbal silence in many societies. Sir James Frazer devotes a chapter to "The Silent Widow" in his study of comparative religions. (13) The Silent Widow belonged to several areas of the world: the Congo, East Africa, British Columbia (Indians), and Australia. The period of silent mourning lasted from a few days to a year, depending upon the tribe. This restriction of silence was based on supernatural beliefs--fears concerning the spirit of the late husband. In Australia, where the mourning period was lengthy, there developed gesture sign languages to continue the necessary communications. Other vocalizations were permitted when language was prohibited. Even laughing was acceptable behavior.

Religious beliefs in Western cultures also contributed to the muting of women. St. Paul admonished women to keep silent in the churches. Indeed, the more conservative congregations took this advice very seriously, and women were simply not allowed to verbalize in the services in any way. Interestingly enough, in Hastings' encyclopaedic study of religion and ethics, a section on silence is included, which covers the worshipful and devotional silence and the monastery silence, but says nothing about the imputation of silence to women. It should not be forgotten that Jesus Christ himself did not instruct women to be silent, but rather encouraged them to participate. Religion must come to grips with the conflicts imposed upon women in order for religion to be viable.

The famous linguist Edward Sapir long ago noted sex-discrimination and rank-discrimination, and the relationship of these two in language. (14) The balance of power is maintained by imposing tyrannies back and forth. Females may respond to male verbal dominance by hen-pecking or by becoming inconsolably emotional. Language differences and nonverbal behavior . . . particularly the latter, might prove to be a measure of discrimination in evaluating relationships in society. Silence is imposed by various "put-downs." These are subtle forms of control, sometimes covert, usually out-of-awareness and reflecting mental states and attitudes.

The "put-downs" can take many forms, some of which we have already discussed in previous chapters. Titles are a way of denoting inferior status and can be used to suggest that females do not have anything of value (to a male) to say or contribute. Labels and descriptors imply unequal status: "girls" would not participate in an adult discussion. Labels can also be used as "the killing abstraction." (15) It has been noted that the label, "Women's Liberation," is being manipulated to create a counter-productive movement, which would again silence women. It is certainly a distraction from one's career to be asked rather pointedly, "Are you a feminist?" While language is a means of communication, it can also be used to misdirect human beings. Flattery is a deceptive put-down; used by a male, it can fool the listener into thinking she is valued highly. One does not hear the blarney poured out to experts and authoritative figures--they don't need it. "Explanations" are very often subtle put-downs. Observation shows that explanations usually go one way: male to female. It is unladylike for a female to explain a concept.

Categories are another way of putting women aside. . . . These status groupings continue to appear. A young man from the riot-filled Sixties defines his situation: "I have a woman, a child, and a dog." A well-known educator in California prides himself: "I get along all too well with small children and with women's clubs." A radio discussion on street loitering listed the "freaks, widows, and kooks." A campus newspaper reviewed a production which satirized social ills such as economics, Women's Liberation, and corrupt government. A social studies textbook wrote that "Men trekked over the mountains with their wives, children and cattle."

So be it. Females, classified thusly, could hardly be in a state of mind to be anything but silent. But, if by some stint of courage, or overflow of knowledge or experience, a female does start to talk, the final put-down is interruption. Two studies have been made in California recently that indicate the high incidence of males' interrupting females, and husbands answering for their wives. "The hypothesis is that women. . . have restricted rights of speakership which [are] reflected in male speakers' overriding the constraints of conversational ordering when engaged in conversation with females." (16) In another study, which focused on visibility in the interaction of two persons, rather unexpected findings resulted: "It looks as if males are motivated to dominate and do so largely by interrupting and talking more, especially when the normal cues for floor-apportionment are absent." (17)

All of these put-downs are socially acceptable, and the necessary aggressive behavior that would challenge and overcome the put-down is socially unacceptable on the part of women. Thus, they are forced into patterns of silence. How and when are these patterns of behavior learned? And how can they be so deeply ingrained, so that even those who know better can hardly overcome the set patterns? One study that focused on the variations that children learn very early in language behavior noted that the first is between speech and silence. "Very small children will frequently talk or jabber nonsense to their own parents or siblings, but fall silent in the presence of strangers." (18) We have already noted that male/female differences start even before this. It is not unreasonable to extrapolate and note that females learn patterns of silence very early.

Tyranny, however, receives its counterbalance. In turn, men are the victims. In place of silence, nagging becomes a way of dealing with a situation--a way of bringing about an equilibrium, albeit a distasteful one. Another way is for the female to focus on her delicacies and purities and not permit the male to express himself emotionally or with "rough talk" around her or the children. George Bernard Shaw recognized that the most tyrannical female was the most resolute opponent of Women's Rights. (19) Thus, both are losers--neither man nor woman permitted full range of expression for every human emotion. The personality is damaged and creativity is curtailed with the inhibitions imposed. When human beings are robbed of their dignity, all humanity is debased.

The truth is that a slave State is always ruled by those who can get round the masters: that is, by the more cunning of the slaves themselves. No fascinating woman ever wants to emancipate her sex: her object is to gather power into the hands of Man, because she knows that she can govern him. She is no more jealous of his nominal supremacy than he himself is jealous of the strength and speed of his horse. The cunning and attractive slave women disguise their strength as womanly weakness, their audacity as womanly timidity, their unscrupulousness as womanly innocence, their impunities as womanly defenselessness; simple men are duped by them, and subtle ones disarmed and intimidated. It is only the proud, straightforward women, who wish, not to govern, but to be free. . . . (20)

We have only partially answered the question of the young man who wondered why women haven't created to the extent that males do. We still have much to learn about the brain, about creativity, about environment and learned behavior, about repression, and about the marriage dynamic. Perhaps the question cannot be satisfactorily answered until it is asked by more women. Perhaps these days we are seeing a start.

An Androgynous Language--The Future Tense

Language Planning

In any behavioral change adjustments with resultant strains and tensions are inevitable. In some instances of behavioral change the strains are so minor one hardly notices them. In the matter of change in male and female patterns, the stakes are inestimably high and the emotions are at a peak level; therefore the strains and tensions are enormous. In situations as potentially explosive as these, persons who lose their sense of humor lose themselves. Extremes of behavior will occasionally occur and there will be seemingly endless chaos. But, as in all changes of behavior, the end result will again be an orderly language system with acceptance and peaceful co-existence. Until we get to that peaceful end, we will, like our immigrant ancestors, wander between two worlds, belonging to neither one nor the other, but working toward that integrated whole which will be our permanent linguistic homeland.

Today dialogues and conversations are taking new forms. Both men and women are learning new styles and modifying linguistic forms they are no longer comfortable with. It is possible to elevate women to equal consideration and respect without putting men down. Some changes taking place today are being accepted in a relatively easy and smooth way. We have all been observing some examples. The several-volumes set of reference books listing important persons in the technical world has for 65 years been known as American Men of Science. Recently the editors changed the title with little or no fanfare, at least to the public, to American Men and Women of Science. The stylebook of the U.S. Government Printing Office now includes "Ms." in its list of acceptable titles. More and more people are using the alternatives "he or she" and "him and her" rather than the lone form "he" or "him" in their speaking and writing. The particular illustrations that I have noted are from men speakers as well as women--men who have important positions such as presiding officer, or mayor, or chancellor. Someone has suggested that the form s/he could be a useful device in the written language. It seems worth trying.

Editors of journals are publishing revised editorial policies advocating change that will "eliminate the subtle forms of discrimination against women which so often are practiced as a matter of course in the process of discussion and writing." The Washington Post issued a memo to the editorial staff: "Words like divorcee, grandmother, blonde or housewife should be avoided in all stories where, if a man were involved, the words divorce, grandfather, blonde or householder would be inapplicable." (21)

Advertisements now include such expressions as, "The officers and crew of S.S. _____ are men and women of gentle manners who speak your language." Government, business, and school forms are being revised with terminology that includes both male and female: "this student" instead of "this man." A news magazine recently substituted "Person of the Year" for "Man of the Year." A newspaper account of a recent betrothal gave equal space to the education and career accomplishments of both bride and groom. A 200-year-old coeducational college is changing the last stanza of its Alma Mater, which says, "Men may come and men may go . . . ever to thy sons a pride" (incidentally, the song was written by an alumnus who had six children--all girls!). A major textbook company has published a "Guidelines for Improving the Image of Women in Textbooks." (22)

Across the nation committees are combing the textbooks and newspapers and suggesting alternate wording such as the following: people, human beings, or person for men/man; telephone lineworker for lineman; seller for salesman; journalist or reporter for newspaperman; clergy for clergyman; police officer for patrolman or policewoman; cabin attendant for stewardess; business people for businessmen; members of Congress for Congressmen; mail carrier for mailman; ordinary people for the common man/the man on the street; a farm couple for the farmer and his wife; homeowners and their children for a homeowner and his family; human energy for manpower; ancestors for forefathers; the women in the office for the office girls.

A different focus is suggested for some phrasing: "Marie Curie did what few people could do" for "Marie Curie did what few people--men or women--could do." "Women in ancient Egypt had considerable control over property" for "The ancient Egyptians allowed women considerable control over property." (23) A news account reported, "A driver was stalled. . . ." instead of "A woman stalled her car. . . ." These changes are being made and suggested by both men and women, it should be noted. Males who are interested in fairness and consideration for all persons have come out with strong statements and guidelines. (24-26)

The examples quoted above are all articulated quite easily and move smoothly into speech acts without major adjustments in linguistic structures and rhythm patterns. There remain some difficult problems. There will be indecision and uncertainty until a solution is worked out for such anomalous forms as "Madame Chairman." The matter of pronoun referent is still a burden. The use of "he" alone is unsatisfactory in view of past history leaving women out of the scene. There needs to be some evidence that the females are in consideration. Suggestions for a new pronoun that does not differentiate have been made on many occasions.

In the last century a serious attempt was made to add another form to the set of referents that would solve the he/she dilemma. Charles Crozat Converse, a musician and lawyer, introduced a new word, "thon," which he constructed from "that one" with the idea that it would take the place of "he" or "she." By 1889 it had received some attention and, Converse claimed, had "gained the consent of many eminent philologists." His efforts made enough impact for the word to be included in the entries of Webster's second edition, where it was glossed as "a proposed genderless pronoun of the third person." Interest faded so completely, however, that the third edition of Webster's did not include it. Other recent terminology has been put forth--all to no avail. The reasons are clearly linguistic, as I observed earlier. These pronouns belong to a set or class of linguistic forms which is not easily expandable. It is relatively easy to introduce new nouns into the language: margarine, polyester, and astronaut. But classes of words such as pronouns remain stable for hundreds of years without significant change. It is easier to change usage than to change the list of items being used. Linguistic changes down through the ages have followed linguistic patterns.

Other changes advocated have been the order of items listed as in couplets: women and men instead of men and women; female and male instead of male and female. This kind of change would also be difficult, I believe, again for linguistic reasons. Rhythm in language is far more important than most people realize--even linguists. These pairs are only a small sample of dozens of pairs in English which have been said as is for hundreds of years and will resist change not because of chauvinism but because the rhythms of accent and timing are familiar. Note other pairs which would also be difficult or impossible to change: bread and butter, meat and potatoes, milk and honey, bride and groom. A pair that does seem amenable to switching is "boys and girls" or "girls and boys." This pair does not seem to follow the strict ordering which the others preserve. Euphony also has a strong influence on what things can be combined and

in what order. "Her or his" does not flow with ease as "his or her" because of the particular combinations of sounds.

Salutations in business letters to companies or unknown persons is also a matter to be dealt with, although few suggestions and little experimentation have been tried. "Dear Sirs" no longer pleases everyone. "Dear Friend" has been offered, but somehow seems out of place to a business establishment. It would be additionally incongruous if the letter were a reproach of some kind. "Gentlepeople" has been suggested, but I suspect with more tongue-in-cheek irony than seriousness. "Ladies and Gentlemen" might accommodate in formal situations where the persons are unspecified or unknown.

Serious and responsible people can experiment in areas that will encourage rational changes based on patterns of human behavior. Such a process may bring stability to the seeming chaos. Research in languages and linguistic studies allied with male/female social behavior should be undertaken in depth. There is much to be done yet in understanding the universalities of gender systems in all languages of the world. We don't even understand the gender system in English yet. In order to analyze and construct theories there is a crucial need for data. It would be useful, perhaps, to know more about the anomalous use of gender systems in other languages besides English. The matter of personification is of vital importance to the study of human beings and their relationship to their environment, and yet few studies have been made in languages of the world on this aspect of animate and inanimate relationship. And what of the languages of the world without gender systems? How do they deal with male/female referents? The matter of selectional restrictions or grammatical categories with regard to male/female conduct has not been explored sufficiently.

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Margaret
Mead



Lifestyles

Multiple Roles

Can a woman be a scientist, spouse, and parent? Can a man? Answers to these questions depend upon one's expectations about appropriate behaviors for men and women.

The behavior expected of a woman carrying out a particular role may be different from the behavior expected of a man in a similar role. For example, some people believe that a woman's primary role is that of spouse and that being a spouse (wife) means she "should" keep house, provide social support to her husband, and further her husband's career. She "should" also be a parent, and being a parent (mother) means she "should" give her full attention to her child. Women are often not expected to assume the employed worker role. If they choose to work outside the home, they "must" also fulfill the role requirements of being a wife and mother. A man "should" also be a spouse and parent, but he may be able simultaneously to fill most of the expectations of the roles of husband, father, and worker by working for pay and advancement. Because the expected activities for being a wife and mother often do not overlap with the expected activities of being a worker, women who work face competing and often more conflicting demands on their time than men.

Although increasingly young women aspire to meet the demands of performing all three roles as traditionally defined, in practice this is very difficult. The competing demands may be avoided in two ways. A woman may either omit some of the roles or redefine the roles so that they complement each other. If a woman decides to remain single or not to have children instead of not working, she may face disapproval from others who hold different norms of appropriate roles for women. However, the traditional norms are changing and are far from universally held, so that the woman who makes a nontraditional choice may now get support from those family members and friends who share norms similar to her own. If she decides to attempt to combine all three roles, but define them differently, she may also face disapproval and lack of social and institutional support. For example, if she decides to pay someone else to care for her child while she pursues a career, she may face a horrified mother-in-law, a dubious boss, and expensive or inconvenient day care facilities. On the other hand, this redefinition of the role of mother may allow a woman to provide for her child's material and emotional welfare more effectively than if she were unhappily confined to the house. The effect that assuming a career has on the performance of the wife and mother roles and the effect of these domestic roles on her career depends on the extent to which the individual woman

and those persons who are important to her define these roles compatibly.

Every woman is faced with the necessity of making her own individual choice of roles and definitions of roles depending on her particular circumstances. The following articles provide information about how some women have defined their roles and the consequent effects this definition has had on their ability to combine the roles of scientist, spouse, and parent successfully. These readings tend to emphasize the issue of combining roles rather than omitting roles. This bias may speak to the majority of women today who are not willing to give up marriage and/or children to ease their career progress, and who often already have a family before they consider their career goals. This bias does not, however, reflect a judgment about the optimum solution to conflict among multiple roles, but rather reflects the paucity of personal accounts and professional research on single or childless professional women. Although many women scientists are single or childless, the strong social pressures against making these particular choices contribute to their infrequency as well as to the relative absence of written accounts of women who have made such choices. The biographies of Maria Mitchell, Ellen Richards, and Florence Sabin in other sections of this book serve as examples of these choices, and the Bibliography on Multiple Roles at the end of this section contains more general references on these options.

As you read these selections you may want to keep the following questions in mind:

1. Which expectations for women's roles do I hold? Which are believed by other people but not by me?
2. What problems may develop in combining multiple roles? How have these problems been solved? Which of these solutions do I prefer? Which solution is supported by a social institution, and which will I have to implement by myself?
3. How many roles do I realistically have the resources to fill at any one time? How can I maximize my resources? Is the worker role the best one to give up?
4. Are the rewards of the lifestyles described here similar to the ones I personally desire?

Two Examples

In the first two articles, Isabelle Karle and Ruth Weiner share their personal experiences as professional and family women. Dr. Karle relates the experience of a crystallographer who is committed to her career while simultaneously providing arrangements for meeting the needs of her husband and children. Dr. Weiner has chosen to fill the role of parent and spouse in a different way, by accepting an opportunity for professional advancement that required her to move to a location some distance from her husband. The differences reflected in these lifestyles suggest that there is more than one satisfying way to combine multiple roles.

CRYSTALLOGRAPHER

by Isabelle L. Karle

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My parents emigrated from Poland and settled in Detroit, Michigan, where I was born. They had a very little formal education but were self-taught through reading a great deal, both in Polish and in English. My mother was a seamstress and my father a painter. They were thrifty and capable people who lived much better than people with comparable incomes because of good management and do-it-yourself projects. Although both came from large families, they limited their children to two. Even through the Depression days, they managed to save enough money for vacations in the country and at the beaches in Michigan, to visit friends and relatives in Wisconsin, Minnesota, and Ontario, and even for a trip to Florida and to New York City.

We lived in a new suburb where the majority of people were immigrants, from a number of European countries. Thus, by contact with neighborhood children, I was introduced to many European cultures. There was little opportunity to observe life as lived by American families whose forebears had lived in the United States for a longer time. Only Polish was spoken in my home. I learned to speak English in school. By the time I entered elementary school, my mother had already taught me to read and write in Polish and to do the basic operations in arithmetic. School work was interesting and easy, and I progressed rapidly. Fifteen and one-half years elapsed between my entering first grade and being awarded the Ph.D. degree.

My parents constantly encouraged my younger brother, who is now an engineer, and me in our school work, even though they understood very

little about science. They would have preferred, at the time, that my goal in life would be teaching or law. My first encounter with chemistry was in high school. One year of science was required for a college preparatory course, and chemistry was chosen by chance. The subject fascinated me, and I decided to pursue the study of chemistry in college. When I mentioned my intentions to my high school chemistry teacher, he tried to discourage me by saying that chemistry was not a proper field for girls. I was awarded a four-year undergraduate scholarship to the University of Michigan and further augmented my finances by obtaining a assistantship in the chemistry department. Although I graduated with highest honors, I was not encouraged in graduate studies at the University of Michigan. The usual manner of supporting graduate students in chemistry, then as now, was by awarding teaching assistantships, and none were available in the chemistry department for girls; however, the American Association of University Women graciously awarded a fellowship to me, and the following year I received a Horace H. Rackham Fellowship. My financial problems were solved partly by good fortune, but mostly by being able to maintain a top-ranking scholarship position and to qualify for the few fellowships that were available.

While I was in graduate school, I married a fellow graduate student in chemistry. In 1946 we were seeking permanent professional positions. Although academia appealed to us, there were no opportunities for women in institutions with reasonable research facilities. We both joined the Naval Research Laboratory, a government institution. The decision has proved to be an excellent one. I have been well supported in research, given opportunities for development, encouraged to attend national and international meetings, and given the opportunity to present the results of my investigations, both in lectures and in publications.

Management of Career and Children

My husband and I have three daughters, born in 1946 (before the beginning of my career with the government), 1950, and 1955. From 1946 to 1959 we had live-in housekeepers for household duties, with emphasis on child care. The housekeepers were generally widowed women from the mountain regions of Virginia whose grown children had come to the Washington, D.C., area. One woman stayed with us for seven years, and the others, for periods of two years each. The eldest daughter began attending nursery school at age three and the youngest at age four. They liked the companionship of many children, the activities of the schools, and the animals, including a goat, kept by the schools. The middle daughter preferred to stay at home with the housekeeper, who spent several hours a day reading to her.

Since 1959 we have managed without household help, except for a woman who comes to clean the house once a week. Accordingly, a reasonable amount of organization has been needed, as well as cooperation from all members of the family. My husband approved of my career and my professional aspirations and offered help with household duties. By nature, both of us are reasonably neat and orderly people, and the

children are taught not to scatter their belongings. The children had small responsibilities, which increased with age. They were taught to look after each other's needs. Clothes, books, lunch money, and any other items needed for school were laid out the evening before, in order to avoid a rush in the morning. Clothing was selected with an eye toward minimum care. If the girls wanted blouses or dresses that required ironing, that was their own responsibility. Grocery shopping was limited to once a week, usually Thursday evening, when the supermarkets had already received their weekend supplies but before the markets became crowded with weekend shoppers. My husband selected all the meat and fish products, one of our daughters would select the staples, and I would choose the fresh produce and dairy products. A list is always kept in the kitchen, so grocery items are recorded for the next shopping trip as they are used up.

Twenty years ago we purchased a plot of land in Virginia and had a house built of brick and thermally insulated aluminum windows for minimum maintenance. All the modern conveniences were included: automatic dishwasher, automatic clothes washer and dryer, garbage disposal, freezer, and oven with automatic controls. The latter is very important for preparing meals in absentia. A roast, potatoes, and/or casserole can be placed in the oven in the morning and the cooked food will be ready at the appointed hour in the evening. One daughter would prepare a salad and another one the dessert, and the youngest would set the table. The lake adjacent to the house has provided recreation such as swimming, boating, fishing, and ice skating.

All members of the family have been quite healthy. Since there has been little illness, I have not had to spend time and energy in nursing duties. Although there has been adequate social life, I have not participated in bridge parties, bowling teams, women's social and philanthropic organizations, cocktail parties, or neighborhood coffee socials. Since many women in our neighborhood have careers such as teaching, nursing, economics, and so on, we understand each other's need for private time and are not offended by any superficial appearance of lack of neighborliness.

Relationship with Children

During the school year, I had restricted travel to one conference, in order not to be absent from home for too long a time. During the summers, as soon as the children reached the age of five, they have accompanied my husband and me to conferences held in various parts of the United States and in foreign countries. The children have been sufficiently independent and knowledgeable to occupy themselves with local museums, parks, and other activities while I was occupied with the business at hand. In this fashion, they have seen most of the United States and are familiar with half the countries in Europe.

Evenings have been and generally are spent at home with the children. My husband and I have always made ourselves available to them.

When needed, help was offered with homework, school projects were discussed, their cultural education was augmented at home, and transportation was provided for evening activities. We have had many cooperative projects at home. The girls have become accomplished seamstresses, cooks, and home decorators.

Our daughters are now old enough so that some assessment can be made of the effect of their home life on their future careers. Each of the girls has been an excellent student. The eldest, who is married to a scientist, is in graduate school at the University of Washington, in Seattle. This summer she expects to complete her requirements toward a Ph.D. degree in physical chemistry. She has already published several scientific papers. Her interests and hobbies, however, encompass a rather wide field of activities. They include playing the piano and clavichord, hiking, ocean sailing, photography of alpine plants, archaeology, and local politics. The middle daughter is a graduate student at Duke University and has begun her research toward a Ph.D. degree in organic chemistry. She also is an author of several scientific publications. Her other interests include sailing, swimming, photography, knitting, and mathematics. The youngest daughter will be a senior in high school. She plans to study some field of science in college. She is otherwise occupied with gymnastics, swimming, and sewing, and being a student aide in the Guidance Office at school.

There is a close family spirit. The older girls visit home often. They write, keep in contact, and visit, when possible, with their surviving grandparents (who live in Detroit and New York), with other close relatives, and with each other.

Professional Relationships

Recognition in the laboratory and in the scientific community was dependent upon ability, perseverance, and productivity in terms of scientific publications, oral presentations at technical meetings, and invitations to lecture nationally and internationally. In the laboratory I did not feel discriminated against because of sex. There was adequate support for my own research program and for travel to scientific conferences. My goals were directed toward being a research scientist. I have not desired and do not seek administrative positions.

In the scientific community in the field of crystal structure analysis, there have been a number of well-known female predecessors, including Dorothy Hodgkins (Nobel laureate), Kathleen Lansdale (England), Caroline MacGillavry (Netherlands), and Dorothy Wrinch, Rose Mooney, and Elizabeth Woods (U.S.). These women have been professors or have held responsible research positions in their institutions. In addition, there are a number of active female contemporaries. Thus the conclusion can be drawn that gender is not a particular problem, although recognition was accorded to these women somewhat more slowly than to their male counterparts. The main problems that arose in my career are common to scientists of both sexes who propose unusual or radical solutions to problems. Much perseverance was needed to prove that the innovations were correct in two different fields of structure analysis.

A large amount of current scientific work is collaborative, in which specialists from different disciplines each contribute his or her special knowledge and abilities to a common problem. My husband has been among my collaborators. We have found it convenient and profitable to divide our labors so that he contributes most of the theory, whereas I concentrate on the practical aspects of application. Currently, a large amount of collaboration exists with other institutions. In one recent problem concerned with the isolation, characterization, synthesis, and possible medical application of a South American frog venom, the cooperating scientists included a herpetologist from the Museum of Natural History in New York, and an organic chemist and biochemist from the National Institutes of Health in Washington and one from the University of Osaka (Japan), chemists involved in syntheses from the technical university in Zurich (Switzerland), and a pharmacologist at the University of Rochester, as well as myself. Although my collaborators have generally been men, there has always been a spirit of friendly cooperation with no apparent sex discrimination. Among my younger associates at my laboratory, there are both young men and young women. At the beginning, my chief relationship with them had been that of an instructor, until they attained a level of independent research. Some have remained in our laboratory; others are now on the faculties of colleges and universities.

My advice to young women who are ambitious, capable, and truly interested in scientific work is to approach their education and their future scientific efforts with quiet determination but without belligerence. Quality leads to success in the long run, resulting in a firmly established basis for recognition. Demands for equality on the basis of sex without full qualification may bring temporary gains but will eventually prove to be a detriment to a complete acceptance of women in the scientific fields.

Employers should recognize that marriage and children are not necessarily deterrents to full-time careers for women. Many older women have managed successful careers along with gracious and satisfying private lives as wives and mothers. Increasing numbers of younger women appear to be succeeding in combining careers with the traditional role for women.

Discussion

WILMA SCOTT HEIDE (president, National Organization for Women, session chairman): Whatever your intent, quite obviously you have been a role model, and, I suspect, for more than your daughters, as well as an excellent one. I am sure our audience would like to know what a crystallographer is, Dr. Karle.

DR. KARLE: A crystallographer is one who studies the interior of crystals, determining which atoms are connected to which, and how they are arranged with respect to each other in the crystal lattice. The work encompasses many fields and the results are used by many types of scientists--physicists, photochemists, and biochemists, for example.

CHEMIST AND "ECO-FREAK"

by Ruth Weiner

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My family background was probably more conducive than most to pursuit of a professional career. I was born in Vienna, Austria; my father holds both an M.D. and a Ph.D. in zoology, and my mother was one of the first women to receive a Ph.D. from the University of Vienna in a science (biology). My two grandfathers were both physicians (my father's father was Mrs. Freud's obstetrician!). In general, my family belonged to the upper-income Jewish society of Vienna; indeed, my paternal grandmother was extremely wealthy. My parents were religious freethinkers, to the extent that they formally disaffiliated with Judaism, and were political liberals. My mother was, until the Anschluss, a Social-Democrat member of the Vienna City Council.

I am the younger of two half-sibs: my brother is the child of my father's first marriage. It was always assumed that he and I would (1) pursue our education through some graduate degree (preferably in a science), (2) speak at least three languages fluently, and (3) play at least one musical instrument quite well. These, I might add, were the normal expectations for children of the social subgroup into which we were born. Our subsequent forced emigration to the U.S., and life there in moderate poverty for the first decade after immigration, made no difference in these expectations. In fact, we lived up to them. My brother is Dean of the College of Humanities at Montclair State College, New Jersey, and was, for several years, Chairman of the Department of Comparative Literature at the University of Massachusetts.

It was always assumed that I would pursue a scientific career, since I showed some talent in that direction, and I was strongly pressed to go into my father's field: medicine. I cannot say that I was encouraged to have professional aspirations, as much as that it was assumed that I would fulfill them. Indeed, throughout childhood I was quite strictly punished for less than superior academic achievement. I was simply too old to change my basic aspirations when I learned that American girls did not, for the most part, share them.

I am a product of the Baltimore, Maryland, public school system, and my recollections are that I fought it every inch of the way. My high school (interestingly, a girls' public high school, one of two such in Baltimore, and with the best academic reputation in the city) discouraged pursuit of scientific professional careers: "Don't you want to go into nursing, instead?" Since I was already on the principal's blacklist because of various political views and general intellectual snobbery and nastiness, this attitude only encouraged me. At the University of Illinois, I was neither encouraged nor discouraged. I chose physics mainly because it was

difficult and challenging, rather than out of any great interest. It never occurred to me until much, much later in life one could enjoy intellectual pursuits and endeavors that weren't difficult.

I married at 18, so that almost all adult professional and social interactions came after marriage for me. My husband also always assumed that I would pursue a professional career, along with him. The question "Was your profession a handicap to love and marriage?" is meaningless for me. I would, however, agree wholeheartedly that marriage has been a handicap to my professional career. The one person who crystallized what is my present professional life is my husband's (and later, my) preceptor for the doctorate, who urged me to do what I wanted and go to graduate school.

The doors began to close to me after graduate school. I am now aware that there is considerable discrimination against women in chemistry at the point of graduate school admissions. I suppose that, at that time, in graduate school, I didn't think about it and accepted it as a normal state of affairs. In my last graduate school year, I did become aware that the reasons given for sex discrimination (girls marry and don't finish, girls don't use their degree even if they do finish, girls have babies and quit) were a part of a vicious circle that included inadequate child care and enormous social pressures. I suggested a counseling service for female graduate students--a suggestion that caused considerable hilarity among my colleagues and professors.

My first child was born six months before I began graduate school; my fourth, one year after the Ph.D. Two of my children were contraceptive accidents, and would not have been born had there been any legal abortion available to me. If we can render one service to all women, everywhere, and especially those with professional aspirations, it is to legalize abortion on request by the pregnant woman. As long as the burden of contraception rests primarily on women, this is especially necessary.

Child care was difficult, and enormously expensive. Johns Hopkins University had a good day care center for children from 18 months on, which was my best experience with child care altogether. Other than this one experience, I always employed housekeepers, and was paying, in the last year of graduate school, 75% of my income for child care. Since my youngest child started first grade, I have employed no one for either child care or housekeeping, nor does my husband, now that we live apart. I did experience a great deal of guilt leaving my children to go to work, most of it, I now realize, brought on by comments of nonworking mothers such as those at nursery school: "Don't you think you are ruining your children's lives?" From my present perspective, now that they are eight through fourteen years old, they seem to be normal children, with a normal relationship toward me and their father. They are, perhaps, more independent and self-sufficient; for example, my thirteen-year-old bicycles to the orthodontist and they always walk to school.

As girls, they have perhaps one unique advantage: their father assumes that they have the same aspirations as do boys and that they would undertake no greater household responsibility than nor exhibit

markedly different behavior from boys their age. Their attitude toward a professional mother is that this is the normal state of affairs for them. Their behaviour and attitudes truly seem to be shaped more by their individual personalities and the way the six of us interact than by whether or not I have a full-time professional job. The two elder ones are extremely "liberated" young women, and are both explicit and vociferous about any sex discrimination that they observe at school or among their friends. I have perhaps been lucky in that they are all sound constitutionally, and are almost never ill. On the other hand, I also encourage them to ignore minor ailments. They have also been, of course, saddled with a mother who would never be "Room Mother," who could not always attend the school Christmas Play, who never made cookies for the bake sale, but does any of this matter in the long run?

I have never made a conscious effort to spend my spare time with my children, but both my husband and I generally do so. We are a close family, and we like to do things such as skiing and traveling as a family. I certainly enjoy my children's company, and this may, in part, be because I have not ever had it for 24 hours a day, day after day after day. Our social life, rather than our children's, is different from that of our colleagues. Because there is little time for household chores (and less inclination to do them) and little interest on either my own or my husband's part in the house and its setting, we live simply and somewhat shabbily. The place is generally a mess; we almost never entertain in the formal sense. Now that we have separate households, this is even more true. Both domiciles are still furnished in "early Salvation Army," and the sum of the two housing payments are about 12% of our combined incomes. If my children could point to one distinguishing feature of their lives as children of professional parents, it is that our housing and lifestyles are well below the expectations in our income level.

When I had completed a year of postdoctoral fellowship, I went with my husband to Denver, where he had found a position at the University of Denver. We had decided that he should look for a job, but, if possible, should take one in a place where I would have opportunities also, and Denver seemed like such a place. In looking for an academic position in the Denver-Boulder-Colorado Springs area, I came on very restrictive and very overt sex discrimination. The University of Denver, where my husband was, would not even consider my application (nepotism, although a father and son had been department members in the recent past). The Colorado School of Mines wrote (although their policy has since changed), "We never hire women in the sciences." In general, the attitude toward a person seeking an academic job in a given place because her family is in that place is very negative. I finally got a position at the University of Colorado School of Medicine as a research associate, paid from grant funds. I really got this only because both the chairman and my subsequent coauthor were Johns Hopkins men themselves. After three years, by a stroke of luck, a position as assistant professor of chemistry at Temple Buell College, a woman's college in Denver, opened up. Since CU would not put me on hard money or a tenure track, I went to Temple Buell. I was considered by my preceptor at Hopkins to be a great success, because

I had a tenure-track teaching position at a small college for women, one that is not, in my opinion, first-rate.

My experience at both the Medical School and Temple Buell demonstrated to me that fulfillment involved more interaction with people than was possible in a purely research position and a larger role in policy-making than was possible as a very junior faculty member. At TBC I was, fortunately, able to have positive power in making decisions about curriculum, new faculty, budget, and so on. It is a small college, and a faculty member's role in decision making depends rather heavily on his or her own initiative. The structure of the chemistry major program there is primarily my creation; there was no chemistry major when I went on the faculty.

My experience at TBC demonstrates the enormous importance of women as role models for women students. Prior to my appointment, all of the physics, chemistry, and mathematics faculty had been men. The highest professional aspiration voiced by any student then was to become a laboratory technician! Not only was I able to encourage the girls to go on toward a professional career, but I convinced my closest colleague in the Department that encouragement was both necessary and worthwhile. Although he was not himself prejudiced in any way, he had failed to realize that most girls need encouragement to overcome the tremendous internal pressures that have been built up in them.

Shortly after we moved to Colorado, and because I really felt somewhat unfulfilled in my work, I became active in what was then called the conservation movement. We are very active, out-of-doors people and enjoy all sorts of wilderness experiences, and in this sphere, I have developed a deep sense of responsibility of man toward the earth, and see a need to change some fundamental human attitudes. I also saw where, as a scientist, I could render some valuable services to the "lay" citizenry concerned about conservation.

In 1965, a group of us founded the Colorado Open Space Council. I have been on the board since its inception, and have served both as secretary (1966-67) and as vice-president (1970-71). In 1969 I became one of the founders and chairman of Colorado Citizens for Clean Air, and held this post until leaving Colorado in 1971. I served for two years as legislative chairman for the Open Space Council, and was appointed by the Governor of Colorado to the Executive Committee of the Colorado Environment Commission 1970 (one of two women on the 56-member Commission and the 11-member Executive Committee). I have given technical testimony on air- and water-resource problems before innumerable state legislative committees, and five times--twice by invitation--before committees of the United States Congress. I have helped write several laws, including the Colorado Air Pollution Control Act, and have received several state awards for my activity in the field of conservation.

In spring of 1970, while on a speaking tour for the Conservation Foundation, I spoke at a luncheon for a citizens' clean-air group in Miami. One member of the audience (whom I did not meet at the time) was Dean of

the College of Arts and Sciences at my present institution--Florida International University. As a result of the speech, I was offered a deanship (which I turned down), and then the chairmanship of the Department of Chemistry, which I ultimately accepted. FIU has made a deliberate effort to hire women for upper-level administrative positions. For this reason, and because of my work in environmental concerns, they wanted me as chairman. My offer from FIU was totally unsolicited by me and came, I might add, as such a complete surprise that I never connected it with the speech I had given there.

What has happened to my family life as a result of my move to Miami has caused a great deal of private and public comment. Having myself been in the position of looking for an academic job while tied to a given geographic location, I did not want to put my husband, now professor of chemistry at the University of Denver, in that position. FIU has no nepotism rule, except that the case where one spouse has direct administrative authority over the other must be considered specially. I felt then, however, and I still feel, that insistence on a "package deal" would be unwise, would set a bad precedent, and would make everyone unhappy. Moreover, we were not sure that we were ready to transfer the entire family permanently from Colorado. Thus, we decided that, for the present, he would remain in Denver with two children, and I would take two with me to Miami.

Our situation certainly focuses a major problem for women (or perhaps one ought to say for married couples) pursuing professional careers. Unless we initiate jobs that are filled by a married couple rather than by a single person, we cannot guarantee a husband and wife equally satisfying professional appointments in the same geographic location, because there are so few positions available. Elimination of nepotism rules is vitally necessary, and will go a long way toward alleviating this situation, but it is no guarantee of good positions for a married couple.

The choice I made is one which a married woman (or man) would make only after many years of marriage, or when contemplating a permanent separation anyway. It has taught me much about the relative roles of married and single people in our society. I am very lonely, but that is largely because almost all social life takes place for couples. In a married society, the woman alone is a social outcast. If we adapted more naturally to the presence of unmarried, mature adults in our social gatherings, they would not be so lonely, and, I am convinced, the present pressure to be married would be greatly alleviated.

I achieved my present position by a fluke, but in part because I had sought and found fulfillment in an area that is now booming: environmental studies. I became an active leader in the conservation movement because opportunities in my chosen professional area--chemistry--were so severely curtailed by sex discrimination. This is the first time since graduate school that I have felt challenged by my job; I enjoy it very much.

The primary advice I would give girls contemplating a professional career is:

1. Be well prepared, but stay flexible. Few women or men end up doing what they had planned while in graduate school.
2. Maintain your humanity in dealing with people and follow your natural instincts, even though some things you do may be labeled "feminine."
3. Be prepared to work very hard, if you plan to have children. Equality in housekeeping roles is something we are still working to attain.
4. Don't sacrifice your ambition, aggressiveness, or intelligence for any of the standard feminine rewards like marriage.
5. Learn to deal casually with, or to ignore, sexism and discrimination.
6. Set high goals for yourself, and set them independently of your sex.
7. If you marry, marry someone in sympathy with those goals.

Influence of Working on Traditional Roles

What will happen to the children, if their mother works?

In this article Lois Hoffman provides a concise answer to this question. Her summary is the result of her experiences as a researcher and a mother. She shows that, in the areas which have been studied, both mother and child benefit when the mother who wants to work works.

THE PROFESSIONAL WOMAN AS MOTHER

by Lois Wladis Hoffman

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In talking about the professional woman as mother, one could deal with the effects of the career on her mothering, the effects of motherhood on her career, or the effects of combining these two roles on her personal satisfactions. Most of my focus will be on the first: what kind of a mother is she, and how have her children turned out? But let me discuss the other two briefly, for they provide an important context for my major points.

The dysfunctions of motherhood for the pursuit of a career have been pointed out by Bailyn, Epstein, White, and others. We are handicapped in our career advancement by geographical restrictions, family obligations, guilt, and prejudice. The husband's career considerations have been given priorities not only because of his insistence, but also because of our acquiescence. We have been assigned, and we have accepted, the major responsibility for child-care and the household operations. As Rossi has indicated, some men also have not pursued their careers with single-minded devotion, but have allowed their family concerns to temper their ambitions. But this is far more true for women. Furthermore, women have had that mixed blessing--the chance to drop out without censure. We have all returned from a bad day at work to a chaotic household and wondered why we ever left the kitchen. We are harassed, overworked, and in desperate need of a housekeeper.

But for all of this, we may have fared better with respect to personal satisfactions than had we chosen one of the alternative paths that were available. This is not to say that there is no room for improvement, but the life that includes a commitment to the several roles--wife, mother, and professional--may be the richest of all. There are a number of recent empirical studies indicating that, despite all of the difficulties, the bright

and educated women who have combined all three look back with considerable satisfaction and a minimum of regrets (Ginzburg; Poloma; Garland; and Birnbaum). Birnbaum, for example, compared a group of mothers who were also faculty members of a large university with unmarried faculty women, and with a group of mothers who had graduated from college with honors but who had pursued neither further education nor a career. The groups were comparable with respect to age--mainly in their early forties. Of the three groups, the nonworking mothers were the ones with the lowest self-esteem and the lowest sense of personal competence, including even sense of competence about child-care skills. These women also felt least attractive, expressed most concern over self-identity issues, and most often indicated feelings of loneliness. The subjects were asked what they felt was missing from their lives and the predominant answer from the two groups of professional women was time, but for the housewives it was challenge and creative involvement. The single women, in this study as in others (Simon, Clark, and Galway), held higher professorial ranks than the married professionals. Both professional groups indicated high self-esteem and a sense of competence, but the single women were lonelier and somewhat less comfortable in their social relationships.

The data indicate then that the woman who has combined a career with marriage and children has not pursued her career with the total undeviating involvement that has characterized men and single women. She has, in many cases, withdrawn from full career commitments when her children were young, returning to these commitments as they matured (Ginzburg; Astin; and Feldman). Her life has had more variety; growing old has not meant simply a diminution of her powers, but instead she has experienced greater flexibility in responding to the various possibilities that life offers at different stages in the life cycle. This may seem a waste from the standpoint of a society oriented exclusively to productivity in work, but to many it seems a more fulfilling life. I am not even sure that it is a waste in terms of the criterion of productivity, for the woman with the combined roles may retain an enthusiasm and creativity in her work even in her very mature years, when many men who have constantly pursued their careers become bored, stagnant, and bureaucratized.

In turning now to the major focus of this presentation--the effects of the mother's professional role on the child--there is a remarkable lack of data. In part, this is a methodological problem. Studies that have examined the effects of maternal employment on the child usually locate a group of children around the same age, half of whom have employed mothers and half of whom do not, and compare the two groups. Since more than half the mothers whose youngest child is in school are employed, finding these groups is no great problem. But to find a group of children of the same age whose mothers have professions is obviously more difficult. In fact, I know of few such studies. We shall have to rely on those that have compared employed and unemployed mothers' children among the highly educated, or middle class. This can be supplemented with studies that have questioned professional mothers but have not obtained direct data on the children.

Ten years ago I published with Ivan Nye a book on the working mother (see references). The working mother had been considered quite a devil and a great deal of the research reported in the book had originally been undertaken in the hope of documenting the ill effects of maternal employment. But the data simply would not cooperate. Examined as a general phenomenon, the standard study with adequate controls yielded no significant differences between the children of working and nonworking women. That should not have been a surprise. Maternal employment is too heterogeneous a variable to study. To see the effects, we have to break it down, for the effects will be different if the mother works out of necessity than if she works out of choice. The effects will be different for young children than for older, for girls than for boys. The hours she works, her child-care and household arrangements, her attitudes about her role, all will be important. But even those studies which introduced such breakdowns found few negative effects and several positive ones. There was one study, however, that suggested a bit of caution.

This was a study that I did of working mothers of elementary school age children. The working mothers were divided into those who liked their work and those who did not. Each was compared with a nonworking-mother family that was matched with respect to social class, size of family, and other pertinent factors. As you can guess, the women who liked their work were more typically--though not exclusively--the middle class, and better educated. The results indicated that when the working mother liked her work she often seemed to feel guilty, and she compensated for her employment to such an extent that she may have even gone too far. For example, her children helped less around the house than did the children of the nonworking mothers. There was much in her relationship with her children that was very positive--she expressed more positive affection, used less coercive discipline, she felt less hostility toward her children and more empathy--but in many cases she was somewhat overindulgent, and the children reflected this in their peer interactions and school performance. They played more with younger children than with their age mates; they were less likely to initiate interaction with their classmates; their academic performance was not up to par. These are almost the only negative effects of maternal employment found in the middle class to date, and they appear to result not from employment per se but from guilt about employment.

With the exception of the finding just mentioned, there is little evidence that maternal employment has a negative effect, and considerable support for the idea that it has a positive effect, particularly on girls. The daughters of working mothers are more likely to choose their mothers as models and as the person they most admire. Adolescent daughters of working mothers, particularly in the middle and upper socioeconomic groups, have been found to be active, autonomous girls who admire their mothers but who are not unusually close-tied to them. For girls of all ages, having a working mother contributes to a concept of the female role that includes less restriction and a wider range of activities, and a self-concept that incorporates these aspects of the female role. They usually approve of maternal employment, plan to work when they grow up and become mothers, and, if they are old enough, they are more often employed themselves (Nye and Hoffman).

A few years ago a study by Goldberg indicated that undergraduate women tended to undervalue journal articles when authorship was attributed to women. Recently, Grace Baruch replicated this study. She found that the tendency to undervalue the works attributed to women was less than in the earlier study, and that the daughters of working mothers showed no such tendency. That is, the daughters of working mothers, unlike those of nonworking mothers, did not assume that women were less competent than men. In this study, the college women who wanted to combine motherhood with a career most often came from homes in which the mothers had successfully combined the two roles. But simply having a mother who was employed was enough to affect the daughter's judgment about the competence of women, and this, of course, implies a positive effect on her self-concept.

If we focus on the daughter's academic and career achievements, we find further evidence of the positive effects of having a professional mother. A number of investigations (Birnbaum; Tangri, Almquist and Angrist; Levine) have found that highly achieving women and women who aspire to careers, particularly to less conventionally feminine careers, are more likely to be the daughters of educated women and the daughters of employed women. (1) The high-achieving woman has a high-achieving daughter. There are several reasons why this may be the case. Various studies suggest that the optimum conditions for a high-achieving female include the presence of a model, and the model of the professional woman is more relevant to academic and achievement goals than is that of the nonemployed housewife. The optimum conditions for achievement also include independence training, which is particularly important because many girls are handicapped by overprotection and encouragement of dependency. And finally, optimum conditions include a good relationship with a father who encourages the girl's independence and achievement while accepting her as a female (Ginzburg; Hoffman). All of these conditions are more likely to exist in the family with a professional mother.

Nor is this simply conjecture. We have already discussed the professional mother as model. Regarding the encouragement of independence, the study by Birnbaum mentioned earlier compared the two groups of mothers, professional women and intelligent nonworking housewives, with respect to their attitudes about their children. The professional mothers indicated pleasure in the child's growing independence. For the nonworking mothers, on the other hand, the child's movement toward independence was disturbing, perhaps because their own importance diminished as the child needed them less. Overprotection by the nonworking mothers was also suggested by the fact that they seemed to worry excessively about their children's health and safety, and they stressed self-sacrifice as a major aspect of motherhood.

With respect to the last-named factor that contributes to the development of achievement orientations in women--having a close relationship with an encouraging and supportive father--there are data also. Many studies have found that the husbands of working women are more actively involved in the care of the children, and other studies show that the active involvement of the father has a positive effect on both male and female children.

Furthermore, the data indicate that the husbands of professional women are more likely to respect competence and achievement in women (Garland; Nye and Hoffman; Dizard; Maccoby; and the Rapoport).

There is very little else by way of solid data that differentiates the children of professional from nonprofessional mothers. For example, I have not discussed the effects on sons simply because we have not yet learned what these effects may be. (2)

There are, however, some hints about possible effects from the studies that have compared professional mothers with able women who are full-time housewives. Several studies, for example, have indicated that the woman who does well in college but does not pursue a career pins her achievement needs on her children, seeking vicarious satisfaction through their achievements (Birnbaum; Tangri). Although her achievement frustrations are not great when her children are young, they tend to increase after a few years (Veroff and Feld). Several studies indicate that about twelve or more years after marriage (3) her need for achievement rises; her self-esteem is low; her feeling of self-sacrifice is high; she is prone to depression; she worries about her competence in general and particularly as a mother; she is very anxious about her children and guilty about occasional losses of her self-control (Birnbaum; Baruch; and Chessler). Despite her eagerness for her children to achieve, she is, as already pointed out, ambivalent about their growing independence. Surely this situation--the plight of the bright, nonprofessional mother when her children have reached school age--does not seem conducive to rearing healthy, competent children of either sex. When the professional mother is feeling harassed and guilty about her employment, she might consider the alternative.

We do not have data on the effects on the child of the various career patterns that professional women have chosen. How important, for example, is the fact that so many professional women have reduced their work load when the children were young? Is the prevalence of this pattern one of the reasons why maternal employment has not been found to have adverse effects? What, indeed, is the effect of the mother's career pursuits on the very young child? We simply do not know. Those who oppose professions for mothers are quick to generalize from the studies of maternal deprivation showing that infants reared in inadequate institutions without any stable one-to-one relationship with an adult suffer serious affective and cognitive deficits. These early and important studies, however, do not provide automatic condemnation of day-care centers, various alternative foster-care arrangements, or even of institutional care itself if the institution is appropriately set up. Research suggests, for example, that putting a small number of babies in the attentive care of a single person mitigates many of the adverse effects. So does increasing the stimulation potential of the physical environment. In any case, the connection between the sterile, understaffed, institutional environment and the setting provided by the professional mother seems remote to me.

On the other hand, we do not know what the effects of the mother's career are on the very young child or what the effects are of the various possible child-care arrangements. No one has studied them.

And now, if you feel that I have painted a bleak picture of the state of our knowledge about the effects of the mother's career on her child, let me add still further to that impression; for much that we know may be obsolete by the recent changes in sex-role prescriptions.

The existing research, such as it is, is for the most part based on women who have been married about fifteen years or more. We had our babies during the time well characterized by the term The Feminine Mystique. From the mid-forties until recently, large families were desired and motherhood was extolled as woman's major role whether she was a professional or not. Freudian theory, or at least the emphasis on the crucial importance of the mother in molding the personality of her child, was at its peak, and the Bowlby and Spitz views on maternal deprivation added a shocking imperative to the mother's presence. This may have made those of us who pursued careers feel guilty, but it may have also alerted us to compensatory responses that had a very positive effect on our children's development. Thus, if the data show, and I think that on the whole they do, that we have done quite well as mothers, maybe it is because we tried extra hard.

On the other hand, we were harassed! Not content with being professionals and mothers, we wanted to be gourmet cooks, hostesses, supportive wives, and femme fatales. The major problem reported by the professional woman in several studies has been the management of the household. The difficulty of finding a housekeeper really was the single most predominant complaint of the women Ph.D.'s studied by Astin. And our husbands may have helped more than the husbands for the nonworking women, but by no means was there equal responsibility for housework and child care (Poloma; Garland).

Much in this situation, however, has changed in recent years, and it is only with great caution that we can generalize from the existing data to the case for the new career women. For one thing, there will be more of them. Data collected each year on incoming freshmen at several colleges and universities show a continual increase in the percentage of women planning to combine careers and motherhood (Cross; University Record). These younger women who will pursue careers may be quite different in values, self-concepts, and expectations.

If they marry, they will very likely marry a different kind of man than any we might have married, one for whom achievement and career lines are much less central. When Horner did her research in 1965 on the motive to avoid success, this motive characterized women, but not men. In an exact replication, at the same school, we found just six years later that men were as likely as women to show evidence of a motive to avoid success. The percentage of men showing this motive had increased from 8% in 1965 to 78% in 1971. The dynamics of this motive in men is probably very different from what it is in women, but the finding does reflect a real change in men's orientation toward academic and career goals. We older women often gave our husband's careers precedence over ours, in part because we felt it was a more intrinsic part of his self-concept. College men today are very different in this respect. This may augur well for the young woman

who wishes to combine the mother and professional roles. The true sharing of household tasks, child care, and career commitment by man and wife may well be a viable possibility, whereas this has been only rarely the case until now (Poloma; Garland; the Rapoport). Furthermore, if these women do become mothers, the styles of mothering may also have changed. Child-care centers may be more available and more acceptable. The pattern of women interrupting their careers for the early child-rearing years may diminish.

Finally, not marrying and not having children may be more psychologically available options than they used to be. In a recent study at the University of Michigan we found that 10% of the undergraduate women said they wanted no children (4) and the same percentage was found by Lozoff at Stanford. This is not a high percentage, but it is considerably higher than previous comparable figures.

These changes must be kept in mind when we consider the data on the professional woman as mother. Very likely it will be easier to combine the two roles, for there will be more social acceptance and institutional supports for doing so. Earlier I indicated that balancing a career commitment with family and affective concerns has resulted for many of us in a richer and more fulfilling life. I think the possibility of achieving such a balance will be greater for the young professional woman of the near future, and for her husband as well. Needless to say, the lives of many men would also be enriched if they too had combined roles.

Footnotes

1. Birnbaum's married professional women are themselves the daughters of educated, employed women. However, the pattern for single professionals is somewhat different, as indicated in both Birnbaum's data and Astin's.
2. There are a few limited studies. For example, in a study of a small sample of gifted boys, the low achievers had more employed mothers, but the high achievers had more professionally employed mothers, and another study which did not separate boys from girls found the children of professional women had better reading skills than a matched sample of full-time housewives (Jones, Lundsteen, and Michael).
3. More specifically, about fifteen years after college graduation.
4. These figures are higher if we include those who said they wanted only adopted children.

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Is it possible to work and keep a husband satisfied? The problem with this frequently asked question is that it places the burden of responsibility for marital satisfaction solely on the woman. Even if the question is asked of both partners, we are still left with the problems of defining satisfaction and trying to measure it. Some of the literature resulting from attempts to do this are reported in the bibliography of this section. A more general report of several survey studies that compared black and white couples has been excerpted here. This report of attitudes and behavior indicates that certain attitudes of both partners may allow the couple to define their own criteria for marital satisfaction and thereby contribute to successful dual-career marriages.

WORKING WIVES: A RACIAL COMPARISON

by Joyce O. Beckett

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What are the attitudes of black and of white husband-wife families about wives working outside the home? Does the behavior of these families reflect their attitudes? Is the propensity of the wives to work influenced by the families' beliefs? Information relevant to these questions has been gleaned from nine survey research studies and from recent census data as well. This article summarizes the research conclusions from these varied data sources.

Survey Research Studies

To gather the type of information needed for studying the problem of working women and for seeing the historical trends in the pattern of women in the labor force, the author reviewed numerous government documents, journals, and books. (1) The aim of this search was to find survey research studies that met the following requirements: data collected in or after 1960 and relevant for husband-wife families, data collected for both blacks and whites and analyzed separately by race, and samples representing various socioeconomic levels. (2)

It was decided to study the intact family, rather than the female-headed or the male-headed family. Varied factors in the family that may affect a woman's propensity to work can be more fully explored, and more information about them may be gathered from the intact family. The husband's attitudes about a working wife, the husband's perception of his wife as an individual, and the extent to which spouses share household

TABLE 1 SUMMARY CHARACTERISTICS OF SELECTED SURVEY RESEARCH STUDIES

Primary Investigator	Date of Data Collection	Area Surveyed	Type of Sample	Number and Type of Subjects	Relevant Information	Where and When Results Were Reported
Leland J. Axelson	1967	Brevard County, Florida	Random Sample of households	n=632 males bn=67 wn=565	Husbands perceptions of working wives	<u>Journal of Marriage and the Family</u> , August 1970
Duran Bell	1967	100 largest metropolitan areas in the United States	Cross-section, national representative sample	n=8,148 husband-wife families bn=2,692 wn=5,456	Differences in labor force participation by black and white married women	<u>Journal of Human Resources</u> , Fall 1974. <u>Rand Paper Series</u> Dec. 1973; and <u>Windsor Conference on Comparative Urban Economics and Development</u> , 1974
Katherine Dickinson	1974	United States	Cross-section, national representative sample	n=1,163 families with children under 12 years of age bn=578 wn=585	Day care arrangements 1975	<u>Five Thousand American Families</u> , 1975
Greg Duncon and C. Russell Hill	1974	United States	National representative sample	n=511 two-parent households, with both parents working in 1972 and having at least one child under 12 years of age	Child care	<u>Five Thousand American Families</u> , 1975

n = number of subjects, bn = number of black subjects, wn = number of white subjects, and on = number of subjects of races other than blacks or whites.

TABLE 1 SUMMARY CHARACTERISTICS OF SELECTED SURVEY RESEARCH STUDIES (cont.)

Primary Investigator	Date of Data Collection	Area Surveyed	Type of Sample	Number and Type of Subjects	Relevant Information	Where and When Results Were Reported
Joseph H. Fichter	1964	United States	Random sample of students who received baccalaureate degrees, Spring 1964, from 77 predominantly black colleges and NORC survey of entire national college graduating class of 1964	n=32,745 (3,543 graduates of predominantly black institutions, 5,282 graduates of predominantly white institutions in the South, and 23,920 graduates of other institutions)	Premarital attitudes about working wives	<u>Graduates of Predominantly Negro Colleges</u> (undated)
Sookon Kim	1967	United States	National Probability sample of wives 30 to 44 years of age living with their husbands	n=3,814 bn=607 wn=3,207	Child care and supply of female labor	<u>Cross Substitution Between Husband and Wife as One of the Factors Determining the Number of Hours of Labor Supplied by Married Women, 1972.</u>
Herbert Parnes et al.	1967	United States	National probability sample of women 30 to 44 years of age	n=5,083 bn=1,390 wn=3,606 on=87	Reasons for women working, attitudes of husbands and wives about working mothers	<u>Dual Careers Vol. 1, Manpower Research Monograph No. 217, 1970</u>

TABLE 1 SUMMARY CHARACTERISTICS OF SELECTED SURVEY RESEARCH STUDIES (cont.)

Primary Investigator	Date of Data Collection	Area Surveyed	Type of Sample	Number and Type of Subjects	Relevant Information	Where and When Results Were Reported
John Scanzoni	1971	Metropolitan areas of Illinois, Ohio, Indiana, Michigan, and Wisconsin	Five-stage probability sample of households in which wives were 18 to 44 years of age	n=3,096 bn=826 wn=2,270	Sex-role norms	<u>Journal of Marriage and the Family</u> , February 1975
Warren Ten Houten	1967	Los Angeles County, California	Multistate probability sample of households with husband, wife, and dependent children	n=1,093 bn=577 wn=516	Parental and conjugal roles in the family	<u>Psychiatry</u> , May 1970

responsibilities--data on factors such as these are available only in a family that has both a husband and a wife. Economic reasons for a wife's working would be expected to be less prevalent in a husband-wife family, since the husband would probably be employed, but other reasons might be stronger. In 1969 the majority of families in the United States were husband-wife families--that is, 88 percent of the white families and 68 percent of the non-white. (3)

Nine studies were found that met all the stated requirements. These selected studies have data from populations that range from county to national samples and that cover different geographical areas; they vary in the date of data collection and in their primary focus of research. (See Table 1.)

Because of the differences in sampling procedures, geographical areas, sources, size of samples, and primary focus, the nine surveys are not statistically comparable. However, they provide pertinent information about family interactions, family attitudes and behavior, and the participation of black and white wives in the labor force.

Attitudes and Behavior

Premarital Expectations. As Reiss notes, premarital expectations influence satisfaction, happiness, and fulfillment in marriage, and they may affect a woman's plans for work after marriage. (4) Most women prefer and expect to combine work with marriage, but blacks are almost twice as likely as whites to do so, and they look forward to working, running a home, and mothering "regularly or occasionally throughout the marriage." (5) White women prefer and expect to work before the children are born and after they are grown. Black women seem to have more confidence than whites do in the ability to pursue an occupation after marriage.

In general, the wives themselves prefer and expect to work more than the husbands prefer and expect them to do so. Black husbands, however, are more than twice as likely as white husbands to prefer that their wives work "regularly or occasionally throughout the marriage." (6) Fichter's racial comparison by sex shows that

...attitudes on marriage, child rearing and wife's occupational role are more dissimilar between male and female white and more similar between male and female Negroes. (7)

Attitudes about Working Wives. Black husbands have more positive attitudes about their wives working than white husbands do. The racial difference persists when socioeconomic status and age variables are controlled. (8) The wives' perceptions of their husbands' attitudes significantly influence their working patterns. The wives who report that their husbands have permissive attitudes have a higher rate of participation in the labor force than do wives who report nonpermissive attitudes. Black wives who report that their husbands have negative attitudes have a higher rate of labor force participation than comparable white wives do.

This suggests that the black wives are less influenced than the white wives by their husbands' negative attitudes. (9) In other words, regardless of what their husbands think and say against their working, they go ahead and work anyway. Black wives are more likely to have permissive attitudes toward working mothers than white wives do.

Family Behavior. Black men are more willing than white men are to make tentative, temporary, or long-range accommodations to the special needs of working wives--for instance, more willing to stay at home with a sick child, to have a smaller family, or to be amenable to the wife's absence from home overnight because of work requirements. (10) Furthermore, black women tend to have greater autonomy, independence, and individualism in their marriages than white women do, and the male-female roles are likely to be less rigid.

Ideology. Black and white families differ in the ideologies they hold. Black families are more traditional in their ideas about family integration; that is, they tend to hold the belief that the husband's interests are more important than the wife's. But in their behavior, black wives and mothers are more egalitarian; that is, they are more likely than whites to believe that their own interests are as important as those of their husbands and children. Whites, in contrast, are more egalitarian than blacks in ideology and more traditional in behavior. (11)

Perceptions and Evaluations. In the wife's self-evaluation and the husband's evaluation of the wife, black women are significantly and consistently rated higher than white women on such items as having intellectual ability, being able to handle a number of responsibilities, being a good hostess, and having the knowledge and experience to hold a job. (12) Black men and women evaluate themselves higher than comparable whites on their ability to fulfill instrumental and expressive family functions. Instrumental functions include the ability to obtain food, clothing, shelter, and other necessities of life; these functions are measured by such attributes as activity, aggression, competition, ambition, and extroversion. Expressive functions refer to the ability to provide nurture and support within the family and are measured by such qualities as warmth, patience, gentleness, dignity, and self-awareness. (13) Black children perceive their parents as fulfilling both types of functions more effectively than white children do. (14)

These racial differences are statistically significant and persist when socioeconomic status is controlled. The influence of socioeconomic status on interaction in the family is greater for white families than for black. The education of black husbands apparently has little effect on the family style and behavior. In contrast, white families in which the husbands are well educated tend to be more egalitarian; white families with husbands of low and average education are likely to be more traditional in the way they live. (15)

Research studies suggest that black women expect to combine the roles of wife, mother, and worker, and prepare to do so. (16) Black men and women view the working wife as a good wife and a good citizen, a

loving mother--a woman who is efficient in household tasks and is better liked by men than are the wives who stay home. (17) These findings do not take into consideration the primary reference group for the particular black family. (18) If that reference group is a black family, there may indeed be less role conflict for the black working wife. However, if the reference group is a professional white family in which the wife is not allowed to work, role conflict and marital disharmony may result for the black family.

Another consequence of the fact that black women have been in the labor force throughout American history may be that some black wives may now prefer to terminate their employment and become homemakers. The black families and reference groups may not understand why. Marital and family conflict may result.

The black woman who chooses to continue working may have concerns about continuing family adjustments that are necessary if she is to remain in the labor force. The employment of black wives increases with age. For white wives the distribution is bimodal, and shows two peak participation methods: the first at the beginning of marriage, and the second when older women return to work after their children have entered school or are grown. Also, the rate of participation in the labor force by wives with preschool children is higher among black than among white wives. (19) Thus more black women and their families than white may be concerned about the adequacy of child care and the arrangements and shifts that may be necessary in the family to provide care for young children.

The white wife's concerns may be different from those of the black wife. The average white wife will probably be concerned about entering or re-entering the labor force and--as with blacks--about the attitude of her husband, the problems of adequate child care, and the effect that her working has on her children. The reference group for the family is also important. Does the family have the traditional American view that a woman's place is in the home or the more liberated view of woman's role? Do the husband and wife differ in their ideology? Do the wife's friends criticize her for working and leaving her children in the care of others?

...Many factors affect spouses' expectations and behavior with respect to their working wives, and one must be able to see how all these factors influence the concerns that the women express. These factors include race, the unique personality of each spouse, interaction between spouses, the length of time married, the number of children, interaction between parents and children, each family member's experiences and stage in the life cycle, opinions of peers and members of the extended family, the reference group for the family, and the view of community groups. (20)

...Many concerns of working wives can be intensified by factors outside the family--for example, insufficient day care facilities, lack of training in marketable skills, and employers' hiring preferences....

New Directions

From this project, a few general implications are obvious. There is a lack of data on the subject of working wives that consider the variable of race. Data from the census and from the selected survey research studies clearly show that the patterns and extent of women's participation in the labor force are different when only black women (and possibly women of other minority groups) are considered than when all women are considered. If this fact is not taken into account, then social policy is based on the pattern of all women--which means it is based on the pattern of the whites, who make up the overwhelming majority of women in the United States.

...A racial analysis of working wives has indicated that black women are more likely to work in spite of racial discrimination and inhibiting demographic variables. Also, research studies indicate that the black family accommodates to the working wife. The accommodations of the family and the working patterns of black women indicate a strong work ethic in the black community.

It would seem that the attitudes and behaviors of black working wives and their families, as revealed in the findings of the selected research studies, might provide the norms and models needed by white families in which the wife works or is thinking of entering the labor force.

Footnotes

1. For a more extensive look at historical data on rates of participation in the labor force by married women, see, for example, Gertrude Bancroft, The American Labor Force: Its Growth and Changing Composition (New York: John Wiley & Sons, 1958); John D. Durand, The Labor Force in the United States, 1890-1960 (New York: Social Science Research Council, 1968); Joyce O. Beckett, "The Labor Force Participation of Black and White Women, 1900-1970," unpublished paper, 1975; Richard Freeman, "Changes in the Labor Market for Black Americans, 1948-1972," Brookings Papers on Economic Activities (Washington, D.C.: Brookings Institution, 1973), pp. 67-132; Abbott L. Ferriss, Indicators of Trends in the Status of American Women (New York: Russell Sage Foundation, 1971); U.S. Bureau of the Census, Statistical Abstract of the United States, 1974 (95th ed.; Washington, D.C.: U.S. Government Printing Office, 1974); and U.S. Bureau of the Census, Historical Statistics of the United States, Colonial Times to 1957 (Washington, D.C.: U.S. Government Printing Office, 1960).

2. At a minimum, a family is made up of husband and wife residing in the same household. The family may or may not have children, and the wife may or may not be working.

3. See U.S. Bureau of the Census, 1970 Census, Employment Status and Work Experience (Washington, D.C.: U.S. Government Printing Office, 1972).

4. See Paul J. Reiss, "Women and Marriage Expectations," in William C. Bier, ed., Women in Modern Life (New York: Fordham University Press, 1968), p. 99.
5. See Joseph H. Fichter, Graduates from Predominantly Negro Colleges, Public Health Service Publication No. 1571 (Washington, D.C.: National Institute of Health), p. 83.
6. Ibid.
7. Ibid, p. 84.
8. See Jon Scanzoni, "Sex Roles, Economic Factors, and Marital Solidarity in Black and White Marriages," Journal of Marriage and the Family 37 (February 1975), pp. 130-144; Leland J. Axelson, "The Working Wife: Differences in the Perception Among Negro and White Males," Journal of Marriage and the Family 32 (August 1970), pp. 457-467; and Parnes et al., op. cit.
9. Herbert S. Parnes et al., Dual Careers, I, Manpower Research Monograph No. 21 (Washington, D.C.: U.S. Department of Labor, 1970).
10. Scanzoni, op. cit., p. 132.
11. Ibid.
12. Ibid., p. 134.
13. Ibid., p. 133.
14. See Warren D. Ten Houten, "The Black Family: Myth and Reality," Psychiatry 33 (May 1970), pp. 145-172; and Scanzoni, op. cit. For a detailed discussion of instrumental and expressive functions, see also Talcott Parsons and Robert F. Bales, Family: Socialization and Interaction Process (Glencoe, Ill.: Free Press, 1955); Scanzoni, op. cit.; and Andrew Billingsley, Black Families in White America (Englewood Cliffs, N.J.: Prentice-Hall, 1968), pp. 21-31.
15. See Ten Houten, op. cit.; and Scanzoni, op. cit.
16. See Fichter, op. cit., p. 83.
17. See Helen M. Hacker, "A Functional Approach to the Gainful Employment of Married Women." Unpublished Ph.D. thesis, Columbia University, 1961.
18. For a discussion of the reference groups that working and non-working women use, see Lena Wright Myers, "Black Women and Self-Esteem," in Rosabeth Kanter and Marcia Millman, eds., Another Voice (New York: Doubleday & Co., 1975) pp. 240-250.
19. 1970 Census, p. 170.
20. For further details about marital expectations and factors affecting satisfaction in marriage, see Reiss, op. cit.

Influence of Traditional Roles on Career Advancement

How do marriage and children affect a woman's career? In the previous articles of this section, the emphasis has been placed on the effect work might have on women's ability to fill their marital and maternal roles. We turn now to the effects that marriage and children might have on the career course of women. In the following two articles, there is one major question to be answered: Why do married women's careers on the average progress more slowly than men's? Is it the expectations of their bosses or the expectations of the women themselves, or both?

DUAL-CAREER MARITAL ADJUSTMENT:

POTENTIAL EFFECTS OF DISCRIMINATORY MANAGERIAL ATTITUDES

by Benson Rosen, Thomas H. Jerdee, and Thomas L. Prestwich

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Within the dual-career family, the husband and wife must negotiate a redefinition of traditional sex roles to cope with the associated stress of their joint career and family obligations. However, in spite of adjustments and redefinitions within the family unit, a number of studies suggest that social and psychological barriers still exist for women attempting to balance family life with a professional career. (See, for example, Arnott, 1972; Bebbington, 1973; Bowman, Worthy, and Greyser, 1965; Epstein, 1970; Kreps, 1972; Rosen and Jerdee, 1973, 1974; Shaw, 1972.) One significant barrier may be the managerial stereotype concerning the appropriate role for women in society. It is quite likely that managers view the principal female role as that of a helpful, supportive wife (Whyte, 1951).

Managerial attitudes that consider the appropriate role for women as that of wife and mother may lead to a number of subtle discriminatory practices that could affect both the career and marital adjustments of working women. It has been alleged that in order to minimize disruption of the female employee's family life, organizations avoid placing women in positions that require extensive travel. Similarly, it has been alleged that female employees are not considered for promotions that would require geographic relocation of their families. Organizations have also been accused of failing to promote women into positions that require greater dedication, on the assumption that most women would put family responsibilities before job demands. Finally, organizations have been accused of expecting female employees to assist their husbands' careers through participation in business

social functions and assumption of childrearing responsibilities, while not expecting similar sacrifices from male employees for the sake of their wives' careers. The end result of these discriminatory practices could be highly frustrating to the working wife who has career aspirations and strong needs for self-realization on the job. Her frustration may be heightened further when she compares her own job situation with that of her husband. The result might be feelings of jealousy and resentment, and a generally increased potential for marital conflict and strain.

The extent to which these subtle discriminatory practices occur has up to now been a matter of conjecture. Accordingly, it is important to determine the actual behavioural effects of managerial sex-role stereotypes, particularly in those situations involving potential conflict between demands and family role. The present research examines how managerial stereotypes are reflected in decisions that discriminate against female employees. Findings are interpreted in terms of their potential effects on dual-career marital adjustment.

Method

Subjects

A questionnaire was mailed to a national sample of 5,000 managers and executives, drawn from the list of subscribers to a well-known business publication. A return rate of 29.5 per cent (1,517) was obtained. As indicated in Table 1, most major industries and job functions are represented in the sample. The number of female respondents (N=75) was quite small, reflecting the poor representation of women in executive and managerial positions (4.9 per cent). Accordingly, only experimental data from male respondents are reported.

TABLE 1. SAMPLE CHARACTERISTICS

Industry	%
Manufacturing	34
Banking, investment, insurance	12
Government, education, social services	11
Construction, mining, oil, transportation, utilities	11
Retail or wholesale trade	8
Management consulting	5
Advertising, media, publishing	3
All other	16
	100
Job Function	%
General management	40
Marketing	14
Finance	10
Accounting	7
Personnel and labor relations	7
Engineering and research	7
All others	15
	100

Procedure

In view of possible influence of social desirability response set on direct questions regarding sex discrimination, a less obtrusive assessment instrument was required. A survey-experiment was developed in the form of an "in-basket" decision-making task. Members of the sample were provided a description of a hypothetical organization and asked to read a series of incidents in the form of letters and memos depicting various organizational problems.

Five of the eleven in-basket incidents were directly concerned with home-career conflicts and are the subject of the present report. (The remaining six incidents depicted problems not involving home-career conflicts.) Each of the incidents was written in two versions so as to manipulate the sex variable. Each respondent received only one version of each incident. For example, half of the respondents receive a "leave of absence request from a female employee named Ruth Brown," while the other half received an identical request from a male employee named Ralph Brown.

The topics covered by the five incidents were as follows:

1. Travel: Evaluations of a married male (female) candidate for a position requiring extensive travel.
2. Moving: Organizational efforts to retain a valuable employee when his (her) spouse's career progress requires relocation.
3. Promotion: Assessments of promotability of an employee who emphasizes that his (her) first duty is to his (her) family.
4. Social Support: Expectations of social support from the spouse of a male (female) employee.
5. Child Care: Receptivity to a male (female) employee's request for a leave of absence to care for his (her) children.

Each of these five in-basket incidents was followed by a series of questions or rating scales designed to measure the respondents' judgments and decisions. For example, in the incident depicting the employee's request for a leave of absence to care for children, respondents were asked to rate the appropriateness of the request on a six-point scale, and then to evaluate two possible responses to the request: granting it without pay, or granting it with full pay.

Two complete versions of the survey questionnaire were assembled. Version 1 had a male employee in the first incident, a female employee in the second incident, etc. Version 2 had a female in the first incident, a male in the second, etc. All of the other details were identical in each incident, with the exception of the employee's first name and other words indicating the employee's sex.

It was expected that the in-basket format would increase the realism of the problems and the readers' involvement with the decision-making task. Responses to the in-basket incidents are comparable to administrative decisions in managerial jobs. Each decision is made for only one employee

(sometimes a male and sometimes a female). It might be reasonable to assume that when managers are confronted with both male and female employees for promotional decisions, leave requests, and so on, they would be likely to act in a fair and impartial way. In such instances, the potential for discrimination would be quite obvious and managers could more easily guard against favoritism.

But in most organizations, administrative actions usually occur as single events, as was the case with the in-basket incidents. In these situations, where the possibility of discrimination is not an obvious factor in the decision process, the influence of sex-role stereotypes would be expected to have greater impact. In summary, the use of two forms of the in-basket questionnaire permitted experimental manipulation of the sex-role stereotypes on managerial decisions.

Since each incident covered a different aspect of home-career conflicts in dual-career marriages, the dependent variables were different for each incident. For this reason, we will present the results obtained for each incident immediately after the description of the incidents, and then discuss implications in the concluding section of the paper.

The Five Incidents and Results

1. Travel: This incident involved evaluation of an applicant for a position requiring extensive travel. It consisted of the following memorandum:

MEMORANDUM TO: Executive Vice President
FROM: Corporate Recruiting Office

Pursuant to our recent discussion with you about the need to recruit a purchasing manager for the new operations, we have developed a set of brief job specifications and have located some candidates who may be suitable for the opening. Will you please review the attached resume and give us your evaluation?

Job Requirements for Purchasing Manager

The major responsibilities of the new purchasing manager will be to purchase fabrics, materials, and clothing accessories (buttons, belts, buckles, zippers, etc.) for the production of finished goods.

For the most part, the purchasing manager will have to travel around the country visiting wholesalers and attending conventions and showings. The person hired for this position should have a knowledge of the quality of raw materials, and the ability to establish a "fair" price for goods purchased in large quantities. The person selected for this position will have to travel at least twenty days each month.

Resume

NAME: Mrs. Karen Wood
 POSITION APPLIED FOR: Purchasing Manager
 AGE: 35 PLACE OF BIRTH: Cleveland, Ohio
 MARITAL STATUS: Married, four children ages
 11, 8, 7, and 4
 EDUCATION: B.S. in Business Administration,
 The Ohio State University
 RELEVANT WORK EXPERIENCE: One year as purchasing trainee,
 Campbell Textile, Inc.
 Ten years experience in various retail
 clothing stores, in sales, buying and general
 management.
 INTERVIEWER'S REMARKS: Good personal appearance,
 seems earnest and convincing.
 Good recommendations from previous employers.

(The alternate form had the resume of Carl Wood, as candidate for purchasing manager.)

Selection decisions and candidate evaluations are shown in Table 2.

TABLE 2. EVALUATIONS OF MALE AND FEMALE
 CANDIDATES FOR JOB REQUIRING TRAVEL

Type of Evaluation	Male Candidate (N = 732)	Female Candidate (N = 744)	χ^2 or t
Acceptability (percent accepting)	31%	20%	$\chi^2 = 16.92^*$
Suitability for the job (mean rating)	3.43	3.01	$t = 5.12^*$
Potential for long service to the organization (mean rating)	3.14	2.73	$t = 5.16^*$
$*p < .01$			

Among respondents who evaluated a male applicant, 31 per cent recommended that the applicant should be hired. Only 20 per cent of the respondents who evaluated a female applicant recommended acceptance. In addition, the male applicant was evaluated as significantly better suited for the job and perceived as significantly more likely to remain with the organization. A majority of respondents felt it quite inappropriate to hire a woman for a managerial position requiring extensive travel.

2. Moving: This incident depicted a conflict between professional careers of husband and wife. It consisted of the following memorandum:

MEMORANDUM TO: Executive Vice President
FROM: Joseph Schmidt,
Computer Operations

As you know, Rachael Cooper is a computer operator in my section. She has played a key role in computerizing our inventory system. Recently, Rachael's husband was offered a very attractive managerial position with a large retail organization on the West Coast. They are seriously considering the move. I told Rachael that she has a very bright future with our organization and it would be a shame for her to pull out just as we are expanding our operations. I sure would hate to lose her now. What do you think we should do about the situation?

(The alternate form of this incident depicted a male computer operator whose wife is offered a position on the West Coast.)

If it is true that managers tend to view the careers of female employees as subservient to the careers of their husbands, the "male" and "female" versions of this incident should elicit different responses from participating managers.

The dependent variables and results for this incident are presented in Table 3.

TABLE 3 MOVING: MEAN EVALUATIONS OF
ALTERNATIVE MANAGERIAL ACTIONS

	Male (N=746)	Female (N=771)	t
Try to convince computer operator that too much has been invested to leave now	4.62	3.23	16.48*
Don't try to influence the computer operator	2.68	4.00	14.03*
Offer a sizeable raise as an incentive to stay	2.88	2.53	4.18*
Try to find an attractive position for spouse with your organization	2.90	2.52	4.09*

*p < .01

The highest rated alternative for males was "Try to convince the computer operator that too much has been invested in his (her) career to leave now." Organizational efforts to retain the computer operator were viewed as significantly more appropriate for a male than for a female employee. A second highly rated alternative, "Don't try to influence the computer operator," was recommended to a much greater extent for a female than for a male employee. Two alternatives that were rated somewhat less appropriate were "Offer the computer operator a sizeable raise as an incentive to stay" and "Try to find an attractive position for spouse with your organization." Ratings of these alternatives also followed the pattern of greater organizational intervention on behalf of retaining a valuable male employee, but significantly less effort to retain a valuable female employee.

3. Promotion. This incident depicted a conflict between job demands and family responsibilities. It consisted of the following memorandum:

MEMORANDUM TO: Executive Vice President
FROM: Mark Taylor--Corporate Personnel Office
SUBJECT: Promotion of Margaret Adams

We are at the point where we must make a decision on the promotion of Margaret Adams of our personnel staff. Margaret is one of the most competent employees in the corporate personnel office and I am convinced that she is capable of handling even more responsibility as Bennett Division Personnel Director. However, I am not altogether certain that she is willing to subordinate time with her family to time on the job, to the extent that may be required with Bennett. I have had the opportunity to explore with her the general problem of family life. She believes her first duty is to her family, and she should manage her time accordingly. This viewpoint has not affected her performance in the past, but it could be a problem in the more demanding role as head of Personnel with the Bennett Division. What do you think we should do?

(Form 2 had the identical memo; however, the candidate was a man.) A summary of respondents' promotion decisions is given in Table 4.

TABLE 4. PROMOTION: RESPONDENTS' PROMOTION DECISIONS*

Recommendation	Male (N=772)	Female (N=727)
	%	%
Do not promote	13	27
Try to persuade candidate to make a stronger commitment before going ahead with the promotion	32	31
Promote candidate based on past performance	55	42

* $\chi^2 = 46.03, p < .001$

In this situation, respondents clearly favored promotion for the male candidate, but not for a female candidate in identical circumstances. The statement, "My first duty is to my family," and the general reluctance to work overtime is interpreted quite differently when attributed to a male employee. A male who desires to separate work and family may be perceived as a wholesome individual striving for a balanced lifestyle. A female employee may be perceived as lacking in loyalty or commitment. A male candidate is more frequently given "the benefit of the doubt" and more frequently given the promotion.

4. Social Support: This incident depicted a situation of marital tension resulting from job-related social requirements. The memorandum was as follows:

MEMORANDUM TO: Executive Vice President
FROM: Judy Garrison, Marketing Staff--Bennett Division

I appreciate the discussion we had the other evening. It was comforting to learn that the problem I am having with my husband is not unusual in managerial ranks. I have taken your suggestion and written up a recent conversation between me and my husband, for your use in your human relations seminar in Chicago. This is, of course, with the understanding that the source of this case will not be revealed to anyone.

I really would appreciate it if you would let me know how you think this situation should be resolved. I'll also be looking forward to hearing from you about the discussion in Chicago. I have titled the attached case "The Cocktail Party."

The Cocktail Party

Jack and Judy Garrison have been married three years. Jack is a very successful free-lance writer and Judy is an aspiring business executive. Below is part of their conversation after attending a cocktail party at the home of an executive in Judy's division.

- Jack: Oh, boy, what a bunch of creeps. Do we have to go to these parties, honey?
- Judy: Jack, honey, you know we have to. These things mean a lot to me. Tonight I had a chance to talk to Mr. Wilson. On the job it would take a week to get an appointment with him. I was able to get across two good ideas I had about our new sales campaign, and I think he was listening.
- Jack: Is Wilson that fat slob who works in marketing, the one with the dull wife? I spent ten minutes with her and I nearly died! She's too much!
- Judy: The people there tonight were so dull I could have cried. Why did I major in English lit, anyhow? I prefer to talk to people who know what is going on in the world, not a bunch of half-wits whose main interest is in their new cars and spoiled kids. I tried to talk to one guy about Virginia Woolf and he didn't even know who she was. These people are incredible. Do we have to go to another cocktail party again next week? I'd like to see "Look Back in Anger" instead. I've got the tickets. One of my husbandly duties is to give you culture. What an uncouth bunch in the business world.
- Judy: One of my wifely ambitions is to get ahead in the business world. You know that these parties are required for bright junior executives coming up in the organization. And I'm a bright junior executive. If we don't go, who knows which one of the other junior execs will get to Wilson with their good ideas.
- Jack: Can't you relax and work a 40-hour week? That's what they pay you for.
- Judy: I guess I'm too ambitious to relax.
- Jack: I'd still like to go to the play. At least we could think about real problems.
- Judy: And I'd be a mediocre, lower management nobody for the rest of my career.
- Jack: I want you to be a success, Judy. But the idea of spending more evenings talking to idiots is too much!

(The alternative form of this incident had Jack Garrison as the aspiring manager and Judy Garrison as the free-lance writer.)

Respondents were asked to choose one of three alternative approaches for resolving the problem. Choices for each version of the incident are shown in Table 5.

TABLE 5. SOCIAL SUPPORT: CONFLICT RESOLUTION RECOMMENDATIONS*

Recommendations	Male (N=772)	Female (N=730)
	%	%
Manager's spouse should go to the parties and stop making an issue of it	69	46
Manager should attend without spouse, even though this would be socially awkward	17	38
Manager should stop attending the parties	14	16

$$*x^2 = 76.38, p < .001$$

When the manager was a male, 69 per cent of the respondents felt that his wife was obligated to attend the social functions, but when the manager was a female, only 46 per cent of the respondents felt that her husband should be obligated to attend social functions in conjunction with his wife's business activities. The pattern of responses to the second alternative is similar. Attending social functions alone, even though socially awkward, was perceived as acceptable for a female manager by 38 per cent of respondents, but for a male manager by only 17 per cent. Very few respondents recommended the third solution (stop attending) for either a male (14 per cent) or a female (16 per cent) manager.

One interpretation of these findings is that there is an implied commitment on the part of a manager's wife to fulfill social obligations associated with her husband's job, but in the reverse situation, there is much less expectation of a social commitment from the husband of a female manager. Different expectations of this sort could enable men to combine more easily their professional and social life, thus giving the appearance of complete dedication to work. Career women may find it more difficult to combine their work and social life and therefore might be evaluated as less dedicated to their work even if they spend just as much time and energy on the job.

5. Child Care: This incident depicted a situation of conflict between career demands and child-care responsibilities. The memorandum was as follows:

MEMORANDUM TO: Executive Vice President
FROM: Richard Bell, Accounting Manager
SUBJECT: Request for Leave of Absence

Ralph Brown, an accountant in the main office, has requested one month's leave beginning next week. He has already taken his vacation this year. He wants the leave in order to take care of his three young children. The day care arrangements the Browns had made for the period covered by this request suddenly fell through, and they have been unable to make other arrangements satisfying their high standards. Ralph's wife is principal of the junior high school and she cannot possibly get time off during the next month.

The problem is that Ralph is the only person experienced in handling the "cost" section in the accounting department. We would either have to transfer an accountant with the same experience from the Richardson Division or else train a replacement for only one month's work. I have urged Ralph to reconsider this request, but he insists on going ahead with it.

I have also checked with the legal department and we do not have to hold the position open for Ralph if he insists on taking the whole month off. I would appreciate it if you could give me your decision on this as soon as possible.

(Form 2 of this item contained a request from a female accountant, Ruth Brown, whose husband was principal of the junior high school.)

Evaluations of the appropriateness of a leave of absence request and evaluations of two administrative responses to the leave request are given in Table 6.

TABLE 6: CHILD CARE: MEAN EVALUATIONS OF REQUEST AND ADMINISTRATIVE RESPONSES

Evaluations of Request	Male (N=772)	Female (N=728)	t
Appropriateness of leave request	2.61	3.64	12.10*
Evaluations of Responses to Request			
Grant leave without pay	3.69	4.25	6.51*
Grant leave with full pay	1.53	1.59	1.56

*p < .01

The leave request was perceived to be significantly less appropriate when it came from a male accountant than when it came from a female accountant. In addition, respondents rated it significantly less appropriate to grant a leave of absence (without pay) to a male compared

to a female. Very few respondents approved of granting a leave of absence with pay to either a male or female employee.

Here we have a situation where the male sex-role stereotype of complete dedication to the job may have influenced respondents' recommendations. It was seen as more inappropriate for males compared to females to take time off for family obligations. Or, it could be that participants felt more concern about protecting the promotability of the male employee. These findings illustrate an interesting dilemma for husbands of career women. Men in these situations may be expected by their wives to share many family responsibilities which have previously been considered the female's obligation. This suggests a need for changes in attitudes with regard to the appropriate role for both males and females in professional and private life.

Summary of Results

The results of this survey-experiment may be summarized as follows:

1. Travel: When a married male applicant was considered for a Purchasing Manager position requiring extensive travel, evaluations were more favorable and decisions to accept more frequent than when the applicant was a married female.
2. Moving: When the husband in a dual-career marriage had an opportunity to move for a better job, his wife, even though a highly-valued employee, was expected to give up her own job for the sake of his career. On the other hand, when the roles were reversed, organizational intervention was considered much more appropriate to retain a valued male employee.
3. Promotion: When a male candidate for promotion to Personnel Director made the statement, "My first duty is to my family," he was still evaluated favorably for the position. An equally qualified female candidate making the same statement was evaluated much less favorably.
4. Social Support: When a male junior executive's wife objected to attending business cocktail parties, the sentiment among managers was that she should stop making an issue of it and go to the parties. When the roles were reversed, there was much less sentiment for the husband to sacrifice and to go to the parties.
5. Child Care: When a male accountant requested a one-month's leave of absence in order to care for his three young children, the request was reviewed less favorably than when a female made the same request.

Discussion

Although it is often suggested that the career plans and decisions of a wife and mother should be determined by negotiation within the family unit, our findings indicate that managers have some rather definite opinions on these matters.

One theme that appears in responses to the in-basket incidents is a degree of managerial skepticism about women's abilities to balance work and family demands.

For example, a married female applicant was less likely to be selected for a job requiring travel than her male counterpart. In another incident, involving a promotional decision, when a conflict was depicted between devoting time to family and job responsibilities, the male was more frequently promoted (and given the opportunity to resolve the conflict), while an equally qualified female applicant was less likely to get the promotion. In addition, the "cocktail party" and "leave of absence" cases illustrate that much less support and sacrifice is expected from the husband of a female employee than from the wife of a male employee in similar circumstances.

A second and closely related theme is that there is somewhat less organizational concern or interest in the careers of women compared to men. Responses indicate a belief that it is more appropriate for the organization to try to retain a valuable male employee compared to an equally qualified female employee.

These findings reflect a pattern of managerial sex-role stereotyping which may have important implications for the marital adjustment of both partners in a dual-career marriage.

Implications appear to be especially great for the wife. Her career progress is frustrated by discriminatory managerial practices. She is confronted at work by the expectation that her professional obligations are clearly secondary to her family obligations, thus putting her in a second-class role. This managerial stereotype may often be shared by her husband. If her professional dedication is neither expected nor welcomed in either quarter, her attempts at role redefinition may lead to increased frustration both at work and at home.

There are other implications centering on the husband's role in dual-career marriages. Attempts at truly collaborative assumptions of family role demands are likely to conflict with employers' expectations and negatively affect the husband's career progress, particularly when he is competing with other men who do not hold similar views in regard to the husband's family obligations in a dual-career marriage.

The end result of current managerial sex-role stereotypes appears to be heightened conflict and stress for both parties in a dual-career marriage who attempt to pursue professional careers but are unwilling to downgrade the importance of family life and child-care responsibilities.

In the past, this problem has often been dealt with by a "tolerance of domestication" on the part of the dual-career wife (Poloma and Garland, 1971), or by an acceptance of greater stress levels on the part of either or both partners (Bebbington, 1973; Hall and Fordon, 1972). A more equitable resolution may depend largely on broadening of managers' sex-role definitions for employees of both sexes. However, until this change in managerial attitudes occurs, an awareness of the nature of the problem may be helpful to marriage counselors and to dual-career marriage partners. Ultimate solution of the problem will require coordinated efforts of the family, employers, and society.

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THE MARRIED PROFESSIONAL WOMAN:
A STUDY IN THE TOLERANCE OF DOMESTICATION

by Margaret M. Poloma and T. Neal Garland

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The fact of discrimination against the woman in our society continues to accrue documentation in social science literature. It is our contention, however, that the thesis on inequality between the sexes is usually incompletely presented. The institutionalized role of women in the family (and the woman's acceptance of her traditional role) may continue to impede their achievement of equality in spite of attempts to change the economic structure of our society. Data from a recent study of dual-profession couples on the woman's desire for an egalitarian family, her career orientation, and her perception of discrimination are used to support the "tolerance of domestication" thesis.*

* * * * *

The 1960's have produced reams of research documenting the discrimination against employed women in American society. . . . The great majority of the articles and books dealing with discrimination against women in our society, however, vastly simplify its complexity. While discrimination is the reported cause of the unequal treatment, this is only a partial explanation. It does not explain why women themselves are often reluctant to accept the research findings reported on discrimination and why women themselves deny ever being victim to discriminatory practices--a phenomenon that we will treat during the course of this paper. We wish to argue that underlying the evidence of discrimination uncovered by researchers is an institution of our family system. It is the family that stands in the way of a woman's career advancement and which may be a major factor in employers' discriminating against women. (1) The demands of home and family make it impossible for married women (and the majority of employed women, both professionals and non-professionals are married) to compete with their male colleagues who are not burdened with the daily routine of homemaking. Moreover, as we shall attempt to document later, research evidence is strong that the young single girl, socialized for the traditional feminine role, is fettered by the marital institution long before she says "I do."

... Marriage (and a young single woman's hopes, plans, and aspirations of achieving marital bliss) continues to make a career undesirable for vast numbers of American women. Poloma and Garland (1971) have

*The study of the dual professional family was carried out with the assistance of the Lena Lake Forest Fellowship granted by the National Association of Business and Professional Women's Clubs, Washington, D.C.

demonstrated how the professionally employed women in their study, by and large, had "jobs" without having the demands and commitments of "careers." Powers' et al. (1969) study of women physicians reveals that married respondents worked fewer hours than their single sisters, and both groups of women practiced fewer hours than their male colleagues. While Simon et al. (1967:230-231) found that the women Ph.D.'s in their study were as productive as their male counterparts, nearly a quarter of their female respondents with children were employed only on a part-time basis and another 16 percent were not employed at all. Almost all those who worked part-time rather than full-time indicated that they preferred it this way because of family responsibilities. Only 6 percent of the unemployed women claimed they were unable to find positions. McFarlane's (1968), Tropman's (1968) and Lewin's (1962) studies all underscore the desire of married social workers for part-time jobs rather than full-time careers. As Rossi (1965) has noted:

Part-time employment is this generation's false panacea for avoiding a more basic change in the relations between men and women, a means whereby, with practically no change in the man's role and minimal change in the woman's, she can continue to be the same wife and mother she has been in the past....

This same theme is evident in research done on career aspirations of high school girls and college coeds. Almquist and Angrist's (1970) and Angrist's (1970) research on young coeds indicated an inverse relationship between career plans and romantic attachments. Their longitudinal four-year study revealed that a girl who had strong career aspirations before meeting the man of her life was likely to show a waning interest in career possibilities after becoming pinned or engaged. Ademek (1970:98), in a review of similar literature on college coeds and career commitment, observes:

Women thus seem oriented to adapting to men rather than to themselves. They generally perceive men, moreover, as valuing women who conform to the traditional female role. Perhaps reacting to this perception, most college women appear to be basically marriage and family oriented and plan their college programs with this in mind. Even the "career oriented" tend to drift toward marriage interests, as they near graduation, both at the high school and college level.

...Yet in spite of the adverse effect marriage and prospects of marriage have on a woman's career, marriage is rarely seen as the villain barring the road to complete feminine equality.

...What we are attempting to do in this article is to insure that the marital dimension becomes part of future writings on discrimination. Van den Berghe (1970:376) observed well when he stated:

Women get screwed, but married women get doubly screwed, if you will pardon the double entendre. And at the risk of exciting the ire of my female colleagues, I suggest that a

good part of the trouble is that so many women like it, or at least play the game rather than buck the system. By that I mean that they let themselves become domesticated and follow their husband's movements. I am not defending the male academic establishment, but I am saying that we academics cannot be blamed qua academics for that part of the sex differential which is due to what we may call female tolerance of domestication.

The implication of Van den Berghe's "tolerance of domestication" goes far beyond the academic world. Empirically it can be demonstrated from data that we have secured in our study of the dual professional family. In a recent study of 53 couples in which the wife was a practicing physician, college professor, or attorney we found little evidence that women desire full equality--if equality means performing on an equal footing with their husbands and holding a man's responsibility for achieving career success. Three findings that have bearing on Van den Berghe's "tolerance of domestication" will be dealt with: (1) the lack of egalitarian family structures among the couples we studied; (2) the fact that most of our female respondents had jobs and not careers; and (3) attitudes of the female respondents on the issue of discrimination. The first two points have been dealt with elsewhere and will only be briefly reviewed here; (2) the third issue will be treated in greater detail.

The Myth of the Egalitarian Family

While the notion of the egalitarian family has been popularized through family textbooks, its actual existence remains to be proven (Safilios-Rothschild, 1969:3). Only one case out of the 53 couples we interviewed could be classified as a truly egalitarian family--a family form which we feel is essential to allow the married professional woman the opportunity for equal achievement and advancement. This case consisted of two professors who made a conscious effort to share (not merely help) in both traditionally male and female role tasks. Both considered it their responsibility to cook, clean, and care for the children. More important, both considered it their responsibility to provide for the family's economic needs; and under no circumstance was the husband's career deemed more important than the wife's. In all other couples, the wife was responsible for the traditional feminine tasks (although usually the husband did "help" in varying degrees) while her husband was responsible for providing the family with status and income. (3) While the degree of responsibilities and helping varied greatly from the traditional (i.e., extremely sex segregated in terms of familial responsibilities) to the neotraditional categories (i.e., couples in which the husband and wife freely assisted each other with the other's traditionally sex-determined family obligations), the fact remains that only one couple could be classified as egalitarian.

A woman professor, married to a physician, provides a typical illustration of the "traditional" family. When Dr. and Mrs. W.'s children were younger, Mrs. W. was not employed (although she held a graduate degree in social work). She was awarded her Ph.D. when her youngest child was 17 years of age, and she now holds a full-time position at the city's leading university. She described her career as being "really and truly

secondary in my book"--secondary to caring for the needs of the family. When asked about the degree of consideration she would give to her career if her husband were to receive a good job offer out of town, she responded:

I wouldn't give any consideration to my position. [WOULD YOU LOOK FOR ANOTHER JOB IN A DIFFERENT CITY IF YOU WERE TO LEAVE THE AREA?] Probably. I can't imagine not being able to find something that I would enjoy in my line of work, and I don't think that I would enjoy doing nothing. I've had a taste of that. What I'm saying, really, is that what I'm doing is out of pure choice, which makes it a great pleasure. I don't have to work for the usual reasons. I suppose I have to work because it is what I like to do.

The respondent viewed her husband's unqualified approval as essential to what she was doing:

I know I wouldn't do it if my husband didn't want me to work. You'd have to have a pretty darn good reason for working if your husband didn't want you to. I can't imagine being married to someone who didn't want me to do what I wanted to do. Even though my career is clearly secondary, I don't feel cheated in any way because I want it this way. If I didn't want it this way, I think the marriage institution as we know it, and as we say it is in fact, in our society would be disrupted and that my marriage wouldn't be a successful one.

She saw her husband as the head of the household asserting, "I think it has to be this way; and if a woman is bucking this all the time, I think something is wrong." She carefully pointed out that her income was not used to provide for the family's needs: "I just use my income for whatever I want--mostly I have invested it. It is never used for living expenses--not for my needs or my family's needs or household needs."

She prided herself on "holding down two jobs" explaining:

I run a household, and a very full household, with marketing and not as much help as I had when the children were small. There are a lot of household responsibilities that I carry now, and my house wouldn't be run any better if I weren't working. And I have a full-time job, which for many men is all they do.

When asked about her husband's assistance in making her work easier, she responded:

He is very insistent about the help situation; he doesn't want me to have any household responsibilities. He feels I do too much on the outside for that. He's very protective of

me as a human being, and I suppose in this way (by having me employ domestic help) he is showing his support.

While the physical care of the home is managed by hired help, this wife still maintains sole responsibility for hiring and firing the help and for performing any of the tasks not completed by the domestic employee. When the responsibilities of child-rearing were greater, she did not work.

The traditional family is thus characterized by four main features:

- 1) The wife's career is equivalent to a "hobby" (a couple of women actually used this term) or viewed on a par with their neighbor's volunteer work. (4)
- 2) The husband is clearly the status-giving and income-earning member of the couple, with the wife's income not being used for family needs.
- 3) The wife's principal role is that of wife and mother and homemaker.
- 4) Hired domestic help generally takes care of the bulk of the routine household chores, with the wife caring for the remainder of the feminine tasks (e.g., entertaining, cooking, marketing, etc.).

In the neotraditional couple, the husband and wife could objectively be seen as sharing the breadwinning role in the family. While in all cases the husband's income was greater than the wife's, the wife was also earning a salary for full-time or nearly full-time professional activity as an attorney, a physician, or a university professor. The question arises, what happens to the family roles when the male is not the uncontested "status-giving" and "income-earning" member of the family and shares this central role with his wife. (5) Twenty-seven of the fifty-three cases were classified as neotraditional, including three couples where the husband was occupying an obviously temporary position as a graduate or professional student.

The neotraditional couples differed from the traditional ones in two interrelated ways: (1) in most cases, the wife's income was needed and utilized to maintain the family's present standard of living and (2) the wife's professional activity assumed a certain importance in any decision the family makes, particularly around the issues of moving to another city and vacation time. It is quite probable that the reason her career assumes an importance is precisely because her income is needed for the family's increased comfort. This does not necessarily mean that both are put into one pot (although in some cases this is the working arrangement), but more often that the wife's income is reserved for luxuries such as expensive family vacations, sending the children to private schools, for an elaborate interior decoration of the home, or to maintain the family's country home. While in the traditional couples, the wife could stop working tomorrow and her income would not be greatly missed (in fact, it sometimes costs the husband money for his wife to work in terms of increased taxes), in most neotraditional couples this is not the case.

Probably the biggest difference between the neotraditional and traditional families cannot be seen in terms of marital role expectation. As with the traditional families, the wife is expected and expects to see that the home is run smoothly, that her husband's needs are met, and that the children are cared for. The husband is expected and expects to be the main provider and status giver. He does not expect his wife to work if she does not want to. The career is her choice. By the same token, he will assist his wife with her domestic role tasks if there is little or no domestic help and if he has no strong personal objections to performing such tasks. (6)

Where the traditional and egalitarian families do differ noticeably is with respect to the importance of the wife's career in making family decisions. While the traditional couples asserted that the wife's profession would be a small factor (if indeed it figured at all) in deciding whether to move or not to move from the city, neotraditional couples felt they would give the wife's career varying degrees of consideration. Two of the younger couples still asserted that they would not move unless both had acceptable positions, but couples who had been in their fields of professional endeavor for longer periods of time were less idealistic. Most women had made a conscious decision to place the husband's career above their own and to follow him wherever he went. The only point of consideration would be whether the wife could find some suitable professional activities in due time. (7)

Careers or Jobs?

While more and more American women are demonstrating that it is possible to combine work and marriage, there is no evidence that they are in a position to combine a career with marital obligations. Our data indicate that even among highly trained professional women, a "career" in the sense of being "personally salient," "having a developmental sequence," and having a "high degree of commitment" (Rapoport and Rapoport, 1969:3) may not be widespread among American women. Most of the women in our sample (especially those with children) did not want "career" responsibilities. The priority of the wife and mother role over professional obligations was expressed over and over in differing ways by the majority of the respondents. A physician working in the academic world stated:

I have succeeded in doing what I want to do--in fact, having achieved academically an assistant professorship, I am rather sure that I do not want any more responsibility. When I was asked to be chairman of our department, I decided this was the kind of thing that would be very difficult for me to balance with my home and marriage responsibilities, so I turned it down.

The respondents were aware of the fact that marriage, and especially children, limited their professional involvement, but they appeared content with the situation the way it was. In exchange for the limitations placed on their career by home and family demands, these wives had financial

security provided by their husbands as breadwinners, while the women themselves enjoyed the option of working or not working. One female physician expressed it in the following way:

This is where I feel that the woman has the tremendous advantage. You can choose what you want to do; and if you don't like doing it, you can stop. You don't have a family to feed.

Some women in our sample did have careers in the fullest sense of the term, but they usually started out as simply working in a profession, often on a part-time basis, in the capacity of a volunteer, or from their homes. One very successful academician divided her work history into the "job" stage and the "career" stage, the latter coming to be when her children were launched and coinciding with the publication of a very successful book. A few respondents moved from a "career" to a "job" or the discontinuance of work completely depending on the stage and the corresponding demands of family life cycle. For example, at least three respondents who were "career-oriented" at this stage of the family cycle will undoubtedly modify their professional involvement as the family increases in size.

Perception of Discrimination

With mounting evidence that women as a group have not achieved a position comparable to men, we were surprised to find that the vast majority of our respondents did not feel that they personally had met with any discriminatory practices. Thirty-seven of the fifty-three women interviewed gave an unqualified "no" to inquiries as to whether they had met with problems in their professions--all of which could be sex-typed as male professions. Many of these women were willing to admit that discrimination may exist, but expressed their conviction that they themselves had never felt inequity's sting. A young and successful attorney observed:

I really haven't felt any discrimination. I know that some women have, but if you really narrow it down, they are the older women. They complain of discrimination. I am not saying that there isn't any, but I haven't felt it.

[A] young resident pathologist admitted that some men are against women training for the medical practice; but again, she refused to see herself as an object of discrimination.

There are a few men who feel that women shouldn't be in medical school because they are likely not to use their training, but I am not sure you can call this "discrimination"--this is just their feeling based on the facts they observe. But then you sometimes have the feeling that things are the other way around and that the instructors are easier on women than on men students.

Many of the women expressed the belief that only the "unfeminine" women in their professions (i.e., the one who swears, takes no interest in her appearance, is overly demanding) have problems of discrimination. A young physician observed:

I am sure the person makes the difference. You have various personalities among women physicians as you have in any situation. I guess I always preferred being treated like a woman first and a physician second. Maybe that has made all the difference.

While the vast majority of the women who expressed the belief that they personally had not experienced discrimination where willing to accept the position that some women have problems, a few refused to see discrimination in this light. Some expressed the belief that incompetence is discriminated against--not the competent woman. A nationally known academic woman stated emphatically:

I have very little patience with those who are constantly trying to document discrimination against women. In the professional world, I don't see it.

The most severe protests regarding discrimination seemed to come from academic women--especially over both the official and unofficial nepotism rules they have encountered. Of the sixteen who complained of some form of discrimination, nine were in academia. One young academic woman observed:

I think that initially my salary had a very important skirt differential in it, but I think that has changed over time and is no longer the case. If a woman wants to make it in a field dominated by men, she has got to make it in her 20's or she will never be taken seriously.

Three of the five second-career women (i.e., women who earned their Ph.D.'s after their children were grown) in academia claimed to experience some form of discrimination in the form of salaries and promotions, lending some empirical support to this young academic's observation.

Yet it should be pointed out that nearly all of the respondents who complained of discrimination had made numerous sacrifices in behalf of their marriages at the expense of their careers. These included taking part-time positions at some stage of the work history, relinquishing fellowships and scholarships, turning down a chairmanship, or being unable to accept a tempting job offer in another location because of the husband's career. Yet, of those who did perceive discrimination, few were willing to blame the institution of marriage for the problems they faced in their professions. The young woman quoted above who had felt that "skirt differential" in her salary had left a Ivy League school to marry and had to finish her Ph.D. at a much less prestigious institution. A lawyer who indicated that her salary was not as good as her male colleagues presently puts in fewer hours and ceased work completely for over two

years at one point of the family life cycle. A new Ph.D. in history found it difficult to get a suitable position--but she did obtain a position (which is more than some of her male colleagues were able to achieve in the tight labor market) in spite of the fact that, due to her husband's work, she could not leave the area. We would suggest that these cases (and we could cite many others) are not so much due to discrimination on the part of the employment structure as it is due to the family structure.

The reluctance of women to admit being victim to discriminatory practices was also noted by Epstein (1968:167) in her study of women attorneys:

We found that regardless of the reality of discrimination, two-thirds of the sample did not feel they had been discriminated against in job hunting: 67% said they had no difficulty in getting their first job and only one quarter reported encountering difficulties in their first efforts to find jobs.

Why the incongruity between statistics on discrimination and a woman's perception of discrimination? Many might argue that a woman's consciousness has not been sufficiently raised--that discriminatory practices are numerous but that a woman is unwilling to see herself as a victim. Another explanation is that a small but highly vocal group of women is highly discriminated against because of their personality characteristics and they interpret this discrimination as sex-based. Perhaps. More likely, however, a woman has been socialized to tolerate domestication and she realized that this may cost something in terms of career success. Yet, our respondents perceived themselves as having the best of two worlds: both of the professional world and of domestic life. Statistics on discrimination are unable to present the women who accept low-paying jobs (such as in family planning clinics or in small private schools) precisely because they are in a position to accept only work which they enjoy. An academic woman provides a good example of this phenomenon--one that we encountered a number of times.

I think there may be some financial discrimination against women, but I am not even sure that's a discrimination really. For example, I can work at a job that pays less because I really enjoy it, because I have a husband who can support the family.... I have taken jobs when they interested me, which a person who had to support a family couldn't have taken because the pay was so low. But the low pay was there; it wasn't changed because I was a woman.

The statistics on discrimination do not sort out situations such as these. In a way, it could be said that married women are in a position to be the last of the genteel professionals (8) --and many of them are unwilling to sacrifice this freedom for the cardiac condition or ulcers which their husbands may be cultivating. They can accept a position because they find the work challenging, exciting, or philanthropic--without worry about pay.

Nor do the statistics on discrimination show the professor who declines a chairmanship because it interferes with her familial obligations, the professional who is employed (or was employed) on a part-time basis in order to accommodate a family, the physician who refuses the directorship of a clinic because the demands of her children are perceived to be too great, or the woman who interrupts her career for five, ten, or more years while her children are young. Yet these are the kinds of situations encountered in varying degrees during the career histories of almost all of our respondents. Could it be that some women are reluctant to complain of discrimination because they themselves realize their professional involvement is not directly analogous to their male colleagues? It would appear that if anything were to "blame," it would be the familial institution in our present culture--and this is something that our respondents were unwilling to alter.

Summary and Conclusion

Far be it from our intention to point an accusing finger at our respondents for their varying degrees of tolerating domestication. The words of deBeauvoir (1953:662) readily come to mind: "Desire for a feminine destiny--husband, home, and children--and the enchantment of love are not always easy to reconcile with the will to succeed." While our high status professional women showed a "will to succeed" far beyond the average American woman, they too apparently must tolerate domestication in order to mesh the roles of career woman with marriage and a family. It is important to point out, however, that these women's own "definition of the situation" were, for the most part, favorable ones. They expressed great satisfaction in being able to combine marriage and a career, with the family role as the salient one and the professional role as supportive in the role hierarchy. To our surprise, this attitude was no different on the part of women 25-30 years of age from those 50 or over.... If we may paraphrase W. I. Thomas's famous definition of the situation, "Our respondents, for the most part, defined their situations positively; and as a consequence, they were satisfied." The wives reported a high degree of satisfaction in combining a career with either a relatively traditional or neotraditional family life. Many of them expressed the belief that they had the "best of both worlds"--the joys of motherhood coupled with a profession which they enjoyed. Most stressed the fact that they did not "have" to work; as Bailyn (1965) has observed, the married professional woman, unlike her husband, can choose whether or not to seek employment. Our respondents reportedly appreciated this freedom to pursue only those avenues of professional involvement which they found stimulating and satisfying.

Changes in the social structure alone will not guarantee equality for the American woman. Certainly the attempt to provide equal opportunities for women and to eliminate discrimination in the professions is indispensable. But there is a realm which, like the feminist movement of the nineteenth and early twentieth century, we have been unwilling to discuss anywhere but in extremely "radical" circles. The realm in question is the need for "revolutionizing our domestic life" (O'Neill, 1969:358). Yet it is only through such a "revolution" that complete equality between the sexes is possible.

Footnotes

¹Vast differences exist between the problems of professionally employed women who are able to derive both intrinsic and extrinsic rewards from their position and women employed in jobs of lesser status and rewards. Many writers have proven themselves oblivious to the nature of their employed working-class sisters' needs. Joan Jordon (1970), herself a working woman who was automated out of her job, has been one of the few writers who demonstrates a solid knowledge and understanding of the employed working-class woman's problems. Other insights have been provided in contributions to Robin Morgan's (1970) volume, *Sisterhood Is Powerful*. The present paper makes no pretense of dealing with all working women. Its thesis was designed primarily with the high-status professionally employed woman in mind.

²For a discussion of "Jobs or Careers?" see Poloma and Garland (1971a). The presentation of "The Myth of the Egalitarian Family" by the same authors (1971b) was also presented at the 85th Annual Meeting of the American Sociological Association, September 1, 1970.

³The exceptions to this statement were the five cases which were classified as "matriarchal." In these couples, the wife's income was essential for the solidly middle-class standing, making it imperative for the wives to work. For the most part, these women resented the compulsion they felt in having to pursue a profession. They wished that either they could reduce their work to part-time positions or that they could cease working entirely.

⁴The exception was an attorney who had begun her career on a volunteer staff and eventually was promoted to a very prestigious position. The wife viewed her work as an important career, but her husband refused to regard her efforts as anything more than voluntary community service.

⁵Even for a position of equal prestige, it was common for the husband to receive a larger salary. One couple reported that the university refused to give both an equal salary (in spite of relatively similar qualifications) because "it was too much money to give to one family." There were five couples in the sample where the wife earned a greater income; these are considered as special cases and were classified as matriarchal.

⁶The use of the wife's income and the husband's ability to be the sole provider may be related to whether or not he will even assist his wife with the household tasks. Recently after a class discussion of sex roles a student commented, "I do a lot of housework on Saturdays while my wife washes and irons. She works all week and needs the help. But I would not do it if her working wasn't necessary to send our boy through college. If she was working just because she wanted to, she would have to take care of the house by herself." We found this same attitude voiced by one of the husbands in our sample who was presently a Ph.D. candidate and his wife was an attorney. He planned to discontinue helping with

household tasks once he earned his degree and secured a position. A number of families reported that the husband helped out with domestic tasks early in the marriage (presumably when her income was greatly needed), but as time continued, he withdrew his help.

⁷One couple was in the process of shifting from the idealistic notion of an equal importance of both careers to the more realistic position of one career's being more important during the time of the interview. A year prior to the interview, the husband had turned down a high status and lucrative position because the wife refused to interrupt her career. Recently, however, she learned that she would not receive tenure nor would her contract be renewed. With a certain bitterness she acknowledged that she would not dare stand in the way of her husband's career plans again if she wanted their marriage to last. She felt she would have to be more realistic about her career goals and would follow him wherever he received his next position. For a further discussion of the importance of the husband's career over his wife's profession, see M. Poloma and T. N. Garland, "Jobs or Careers: The Case of the Professionally Employed Woman" (1970a).

⁸In discussing our findings informally with Warren Breed, Professor Breed suggested the comparison of married women in professions with the gentility found in the professions in an earlier area. We felt this analogy was most intriguing and worthy of further study and consideration.

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Coping Strategies

What can be done to ease the stress of doing "everything?" The following excerpt from Woman's Place: Options and Limits by Cynthia Epstein presents an outline of possible strategies for reducing the stress inherent in filling the multiple roles of worker, wife, and mother. Although precise sociological terminology is used to outline these tactics, the examples provide illustrations of the coping alternatives that are available. In this article the term role refers to socially prescribed behavior and status refers to one's socially defined position. A person performs a role and occupies a status.

RECONCILIATION OF WOMEN'S ROLES: PATHS AND OBSTACLES

by Cynthia Fuchs Epstein

Reprinted from Woman's Place: Options and Limits in Professional Careers.
Berkeley: University of California Press, 1970, pp. 86-150.

Women and Careers

Much of the strain experienced by the woman who attempts to work is structured strain, caused by a combination of an overdemanding set of role obligations, lack of consensus as to the hierarchy of obligations, and the clash of obligations from home and occupational statuses. (1)

However, there are women who manage to fulfill a difficult combination of statuses. At the very least, they perform the basic role demands of each, though levels of efficiency and competence vary. The sets of mechanisms used to meet these demands may be categorized as follows: (2)

The elimination of social relationships. The professional woman may deal with overdemanding and conflicting role obligations by eliminating social contact with specific persons, (3) not roles . . . generate difficulties in the role relationship. Although one may choose to marry and acquire the husband-wife role relationship, who the husband is and how he chooses to play the husband's role may make a considerable difference in whether or not the wife is placed under strain. The professional woman would do well to avoid the persons around her whose expectations cause most inconvenience to and most pressure on her. Included are those who would activate inappropriate statuses in the course of a task. For example, the friend who helps out by caring for the children one afternoon but who makes nasty comments about women who work, or the colleague in a law firm who thinks women ought to stay home and have no head for legal work.

Choosing friends and colleagues is important. The woman does best to surround herself with a circle who share professional and family statuses--other working mothers. If possible, she ought to have an overlapping person set of friends who are also coworkers. With respect to biased employers, she faces fewer obstacles if she avoids any firm known to object to women employees; if she were to be employed by them, she would be faced with many assumptions about her performance based on her status as a woman rather than as a serious worker.

Reduction of the total number or amount of contacts involved in the role relationship (not elimination of the status or the role relationship altogether). For example, a woman is a mother whether she has one, two, or three children, or more. Yet the number of children she has makes a substantial difference in terms of the total demands on her. (4) She may reduce the potential overload of strain by choosing to have a small family. Contrary to folktales that two children are no harder to handle than one, each additional person makes additional demands. Three children means three birthday parties, three school plays, and three sets of clothing to launder; the percentage of output necessary may decrease with each child, but there is an absolute increase in time demands. The same may be true for the number of friends and many other relationships.

The number of statuses in the woman professional's status set. Although expansion of the role set is a mechanism by which some reduce strain, the possession of a large number of statuses which entail real time allocations (as opposed to nominal statuses) can be a serious problem. If a woman can maintain a fairly small number of statuses, she will be less deflected in the performance of her professional and home roles. This means that with the increase of obligations in one status (such as birth of a second child), a woman may have to reduce those of another (perhaps eliminating participation in voluntary or even professional organization). To some extent she is playing a zero-sum game, that is, in order to perform adequately in one role, she must perform less than adequately in another.

The mechanism of redefinition. The woman may redefine her occupational status as being an adjunct to her family statuses. In this way she not only overcomes psychological feelings of guilt, but also may legitimate her occupational role, both to herself and her family. Many women with active professional careers form partnerships with their husbands. There are many obvious advantages to this, not the least of which is that the woman who is in partnership with her husband may define her work goal as "helping her husband," rather than advancing her own career. Since it is culturally valued for the wife to be a helpmate, her occupational activity will not be considered self-indulgent. Similarly, if she defines her motives for working as economically essential for the family, she receives legitimation for "doing it for the family."

Such redefinition of role behavior may not only reduce strain considerably, but it also has the negative consequence of undermining the woman's occupational commitment, circumscribing her activities and limiting her to areas and subspecialties which do not lend themselves to redefinition. A

woman who is a law partner with her husband may say she is only helping him, but she can hardly aim at a judgeship on the same ground.

Intermittent activation of statuses. (5) The woman who can do one job at a time, that is, be a mother at home and a doctor in the office without allowing any overlap, eliminates a lot of problems. Although not all women can rely on this mechanism, depending on the contextual setting it can be used flexibly as needed. For example, if the woman has children of summer-camp age, a decision to send them to camp means she can be temporarily relieved of the mother status. For some persons, among them writers, painters, and academic women who do not have time for research in the winter, this time may be crucial.

Similarly, if she chooses an occupation with slow periods and vacations, she may have more opportunity to activate her mother status. As a teacher she has summers free to be with children; as a psychiatrist she has a month or two of vacation; if she is self-employed as a lawyer or free-lance writer, she may manipulate her schedule throughout the year to activate the status more needed at any given time.

Compartmentalization by scheduling. The woman might adopt a rigid nine-to-five work schedule permitting no evening or weekend activities, or she might arrange work so that it was close to home to permit better and more coordinated work-home activities. It may well be that a woman's facility in being able to suspend one status while engaged in activation of another would be of even more importance in dealing with role strain than it would be for the man. His statuses are apt to have more congruent demands and his occupational role usually has a clear priority. Thus, for the female doctor to be her husband's social hostess for an evening (to be a wife and exclude her occupational status) is of great importance if she wants to meet expectations attached to her wife-mother roles without intrusion from her professional life. The woman who is in the limelight at a party because she is pretty and charming has far less trouble with her husband than the one who holds an audience by her recitation of business coups.

Delegation of tasks and roles. The woman who chooses excellent (although it may be expensive) help for her home tasks has a better chance of performing both occupational and family roles well than the woman who relies on part-time or nonprofessional help. In addition, the more help she has (though probably there is a point of diminishing return), the less strain she will be under. The most successful working women interviewed employed two in help so that, as they explained, each of the two could be relied upon to cover essential tasks if the other became ill. For the mechanism to work, delegation must be professional; selection must be routinized and quality evaluated in advance. Haphazard arrangements only add to strain, although any one crisis may come out all right. In a study conducted for the Bureau of Vocational Information in 1925, Collier found that 42 percent of the one hundred professional and business mothers she interviewed had two or more full-time domestic servants to maintain their homes and take care of their children during the day; only nine of these one hundred women had no full-time servants, and five of them had their

mothers living with them. (6) Although there are no comparable statistics on contemporary professional women (other than the fact that they are more apt to pay for help than women in lower income brackets), observations indicate, and statistics on the decline in the domestic labor force confirm, that contemporary business and professional women have less than professional domestic-help arrangements. (7) Younger middle-class women today, carrying the standard of equality, find it difficult to employ and use servants efficiently.

In addition, we would expect to find that professional women delegate much of the consumer role to others. We would expect them to exhibit less reoccupation with amassing "things."

The increasing observability of role demands. (8) If the professional woman becomes well known in a community because of organizational activity, as in the P.T.A. or a local political club, she may be excused from being asked to do any number of friend-neighbor-community and even family tasks because people "know how busy she is." The main danger of this mechanism is overextension and consequent loss of control over time and social obligations.

Expansion of the role set is effective, however, only if the woman permits the system to help her. . . . Many women in professional life complicate their time budgets by insisting on fulfilling all the obligations attached to their various roles (including much participation in school organizations and extensive family activities) to prove that they are successful wives and mothers as well as professional women. Where such a woman does not limit the demands on her by permitting herself to be excused from many of them, she is apt to experience considerable strain.

Reliance on rules (or appeals to "third parties") for legitimation of role behavior. As in bureaucracies, where managers may use rules to depersonalize a command or reproach, as in referring to a command as coming from "above," women too may use work rules when challenged at home. In a sense this is an aspect of delegation. The publisher's deadline for the woman writer may impress her husband so that he relaxes his social schedule to permit her more time to meet the demands of the outside rules. The obligation to meet office hours or class hours are similar outside rules family members respect and defer to.

Woman and Careers: Social Structures That Help

In addition to the personal decisions individual women may make to resolve role conflict, there are built-in social structural arrangements which may reduce the possibility of roll strain and make it easier for women to perform their professional roles. Always the context in which each occurs is important because, depending on specific circumstances, the same arrangements that help one woman may hinder another's attempt to work.

Shared Statuses

To the extent that women share statuses with others in their role set they may be expected to suffer less role strain than those who share fewer statuses. In the following examples, shared statuses reduce role strain.

The woman who shares statuses (other than sex) with colleagues and clients. If a woman is a member of a Wall Street law firm, her presence probably will arouse less resistance if she, like her colleagues, is white, Protestant, from an upper-class background, a graduate of an Ivy League college and law school, and a resident of an upper-class community. This would apply as well to male members of the firm, but the appropriate complement of personal characteristics may be more crucial for the woman. The tradition-bound gatekeepers of the old law firms who object to women attorneys probably are more comfortable with a woman who is "one of them" in all ways except sex. The same is true for women professionals in small localities who share the ethnic status and school and neighborhood background of their clients; this makes the building of a professional practice easier and ongoing interpersonal relations less strained. Shared statuses also make for greater visibility of the woman's problems. Since the obligations of her other statuses are known to those people with whom she is in interaction, they may be more aware of her need for flexibility in professional role performance.

Shared practices: The woman who works with her husband. If spouses are aware of the duties each must perform during the work day, the demands of work, home, and social life will be clear to both and they can cope with them in a fluid and workable way. There are, of course, couples who find it difficult to work together, although most women lawyers and doctors with whom I have spoken report enthusiastically on work relationships with husbands and a few couples in scientific research have corroborated this. In all of these cases, the husband is the acknowledged head of the team.

Shared Fields: The woman whose husband is in the same or similar field. Bailyn suggests that "the circumstance of a woman's working professionally in the same field as her husband . . . reduces the burdens of keeping up with the field, of maintaining contact within it, and decreases the need for an independent institutional affiliation." (9) The advantages may accrue to both partners: many doctors who marry nurses benefit by their shared orientation to the medical field and their knowledge of the work-a-day demands within it. But these benefits may be offset. Many colleges and universities have rules which prohibit husbands and wives from working in the same department or the same school, and in such situations the wife will usually defer to her husband's career.

The woman whose friends are in a similar field. If friends are colleagues the woman's professional role will be reinforced. Although it seems to be usual for professional women to seek friends among their female colleagues, few may be near at hand and it may be difficult for the professional woman to locate a congenial companion. Soviet women may find it easier to find female friends in their field, for Soviet students are assigned

dormitory accommodations according to academic discipline (10) and are more likely to form permanent professional friendships in school.

Defining Statuses as Complementary

Less role strain will exist where the statuses within the status set are complementary by definition.

The woman may avoid role conflict entirely where there is cultural approval of her statuses and they are defined as being complementary. The case is clear where women choose to practice in the so-called feminine fields. Bailyn has outlined some of the positive consequences of performing professional work "devoted to problems and concerns that the woman herself faces in her private capacity within the family." Some of these fields are education, pediatrics, certain forms of psychiatry, social work, and, of course, nursing. She asserts: "By lessening the distance between her two roles and hence reducing the contradictions in her self-image it would be expected to ease her problems." But, she adds: "It might also lead to a weakening of her intellectual forces by blurring the distinction between work and family life, and create special disturbances in her family role by introducing a degree of external authority incommensurate with the natural conduct of that role." (11) There is no doubt, however, that a girl will meet more encouragement for training in these fields, and even receive genuine assistance in working in them.

Role Relaxation

Where the obligations of one or more statuses are under-demanding, the possibility of role strain will be reduced.

Women sometimes find that the obligations attached to one or more of their statuses do not subject them to many pressures. Probably the woman who is a wife but has few wifely duties has the most freedom to pursue a career. (Just as those who have low-pressure jobs, not "careers," can probably pursue marriage with more dedication.) Since she is married she need not pursue a husband. But she may find that she can be a satisfactory wife without the investment of much time or energy, for example where the husband's work does not require his wife's assistance as hostess or helpmate, or in situations in which work takes so much of the husband's time that she is freed, without guilt, to pursue her own career. Of course, the absent husband makes the fewest demands. War, adventure, and some kinds of jobs may carry the man off for long periods, permitting the woman to be a wife yet relieving her of many wifely obligations. On the other hand, if the husband travels extensively, the wife may find that her burden increases because she may have to be responsible for a great many family obligations for which help is usually provided by the husband.

When children go out of town to college or camp, the woman similarly benefits from role relaxation. She may find that her active role as daughter may diminish if parents move away or travel, or if siblings agree to share parental responsibilities.

Statuses in Name Only

The possibility of role strain will be reduced where a status is nominal rather than active.

This is the polar extreme of role relaxation; the best-known type is the wife-in-name-only, the woman who holds the status of wife but is not involved in it, and sometimes chooses to over-participate in her status as a professional. Some observers claim that women who turn to their work as a primary source of rewards do so because they have failed to find fulfillment as wives and mothers. They are accused of manifesting the psychological mechanism of compensation. Ruth Benedict, for example, according to Jessie Bernard, was escaping from an uncongenial marriage in her work. (12)

It is probably true that, faced with an unsatisfactory marriage, a woman, like a man, may seek compensation in work for the socio-emotional rewards she would normally have within the family. Some writers refer to the family as a tension-management unit of society (13), but it clearly can also be a tension-provoking sector.

It is probably more difficult for a woman to maintain a bad marriage and the demands of work than it is a man. As long as the husband is present, the woman probably needs his good feelings and cooperation to combine her career and home obligations successfully; he can probably manage without her cooperation. Thus, we might expect to find that the marriages of professional women are either very successful or very bad. There are no statistics on successful marriages, but we know there is a higher divorce rate (role-strain resolution by elimination of a status) among professional women than among professional men.

Status Substitutes

The possibility of role strain will be reduced where the obligations on one or more statuses are assumed by surrogates who share enough attributes to be genuine substitutes.

The working woman who has a devoted mother or maiden aunt willing and competent to care for her child has a built-in, reliable and inexpensive surrogate-mother. The grandmother-caretaker offers still other fringe benefits: she is devoted and loyal only to this family and loves the child. She is probably of equal social rank and is not likely to expose the child to improper manners, tastes, and attitudes (exception made for generational conflicts which occur in cases where grandmothers were immigrants to the United States). No opprobrium is attached to her caretaker role because she is not considered a domestic. Further, the mother will probably not feel jealous of her child's love for its grandmother since this is defined as a special relationship which should not be competitive. Of deep importance is the fact that the grandmother, co-opted (14) to the daughter's career plan, will probably provide emotional support and approval.

Highly Ranked Statuses

The higher the rank of the statuses in a woman's status set, the more easily she may be able to manage a greater number of statuses. This is probably true whether a total evaluation of rank (adding the rank of each status) of all statuses is used, or simply the rank of the occupational status (of the woman alone).

Rossi points out that "the higher the job level, the greater is the flexibility possible in the allocation of time . . . to be able to manipulate her hours freely is a very great asset to a woman holding a job, rearing children and homemaking." (15) Furthermore, the higher the social class from which a woman comes, the more freedom she has (through access to money, facilities, and servants) to pursue her own interests. (Riesman points out that in many tradition-oriented societies, certain individuals are encouraged toward individuality from childhood, especially if they belong to families of high status.) (16) Similarly, if the woman is of high social class, she is psychologically freer and more accustomed to having and using the services of others. In short, her social-class status may supersede her female status. This has been offered as an explanation by Riesman for the participation in political life of many upper-class women in societies which generally discriminate against women far more than does the United States. In Pakistan, India, and Ceylon, women have achieved much higher positions than in the Western world. The upper-class person may furthermore be permitted more bizarre behavior than the middle-class person; this is of some consequence when the woman's professional activity is classified as deviant and she must be psychologically free to pursue her goals.

Riesman has also commented: "It seems possible that upper-class position--and I mean here aristocratic values and not simply more money--may give a few academic women a relative freedom from middle-class and lower-middle class conventionalities concerning sex roles. It is clear . . . that academic women, like college women in general, are of higher social origins than their male counterparts." (17) The late Queen Elisabeth of Belgium (1876-1965) is a good example. The queen held an M.D. degree from the University of Leipzig, earned before she became the wife of the future King Albert. The role demands of a queen are high and she did not, as a consequence, practice medicine after her marriage, although she used her training to foster the improvement of health conditions in her country. (In addition to her high upper-class background she also had a substantial role model in the person of her father, Karl Theodor, Duke of Bavaria, who was an ophthalmologist.) (18)

Roles that Reinforce Each Other

In sum, where the network of role relations is supportive, role strain will be eliminated.

In the context of occupational choice and career development. Hall has shown that family and friends play a significant role in envisaging the career line (19) and reinforcing the efforts of the recruit. He finds that

they give encouragement, help establish appropriate routines, arrange for work privacy, discourage anomalous behavior, and define day-to-day rewards. (20)

Further, the supportive network feeds into the professional socialization process. Davis and Oleson have reported in their study of nurses that the significant others who accept and treat the woman as a professional help her to become secure in her occupational identity. (21)

Katz and Hazarsfeld's study found that "whether a person influenced another did not depend only upon the relation between the two, but also upon the manner in which they were imbedded into circles of friends, relatives or coworkers." (22) A Russian exchange student, M. Koslov, told the Bryn Mawr conference on Soviet women that friendship supports were important to Soviet women. She reported that when a woman dropped out of school, friends mobilized, pressuring her to return and offering assistance in the form of study-help.

Although a supportive structure is necessary for a man's success, women may depend on it even more, because of lack of firm initial motivation and inner direction. Even where early influences are strong, many pulls exist to divert women from careers at later states of their lives.

In the context of the home and family. An optimum career situation is most possible where family relations are in harmony, where the spouse favors the wife working and the children are in accord or at least indifferent to it. The woman is dependent on the emotional and practical support of all family members. They must often assume responsibilities that are culturally assigned to the woman and give up services that she can be legitimately asked to provide. Thus, it might be assumed that successful marriages, as measured by lack of friction between the spouses and an agreed division of labor, make a career for the woman a more viable possibility.

Roles Which Are Clear-Cut

The more role obligations are clearly defined, specific, and structured, the easier it will be to perform the role demands of a number of statuses. Certain occupations (and specialties within them) have structures that make the required tasks so specific that decisions as to whether one can or cannot perform them can be made even by the new recruit (23), and the burden of constant and repeated time and resource allocation is eliminated. In medicine, for instance, office hours and appointments with patients are clearly defined obligations. So are the class hours of the academic women, or the sequential time schedule of the female scientist involved in a biological experiment. This regularity also legitimates the time needed to prepare for work. (24) The pressures of meeting a court calendar, deadlines for submission of briefs, writs, and so on, define the time commitment necessary to fulfill the occupational role of lawyer.

In addition, curing the patient, or winning the case gives the professional a clear knowledge that he has finished a task and is free to go on to another occupational activity or fulfill the obligations of another status. This is not so, for example, for the research sociologist, who usually never feels finished with a problem. The researcher further suffers from imprecise deadlines and unfocused sanctions, so that his tasks easily can be deferred to meet the demands of another role when they become pressing. The woman whose job holds strong and immediate sanctions (forfeiture by not appearing in court, losing the case, not getting paid) is able to organize her time more effectively than the woman whose job permits time encroachments from her other roles (the novelist or painter, for example).

This is equally true for decisions involved in the mother's role: if a child is ill and needs hospitalization there is no question but that the mother will leave her job and go to him (and no colleague will challenge her right and duty to do so), but if the child demands constant daily attention with minor complaints the mother may face a fresh and enervating decision each day.

The husband who tells his wife he does not object to her working if "she does what she thinks is right for him and the children" places more of an obstacle in her way than if he had made specific requests (such as two dinner parties a month for his clients and full responsibility for getting the children to school). The husband-wife relationship is such that a full detailing of tasks may not easily be made, but an attempt to regularize and standardize them would dispel ambivalence and the psychic toll resulting from role strain.

Status Obligations Which Bend to the Times

Where the burdens people face in fulfilling status obligations are highly visible, and society places a high priority on them, the institutional structures in which the tasks need to be done will bend to meet personal needs.

When shortages of manpower occur, for example, institutions which must employ women adjust their occupational demands to permit women to meet the demands of their home lives simultaneously. Examples are all-day nurseries attached to factories, more common in Europe than in the United States. In response both to their needs and to public notice of the problem, some American institutions have begun making provision for the children of their professional women staffs. In meeting the needs of women doctors, New York Medical College (The Metropolitan Hospital Center) has initiated a new program permitting part-time residence at the Department of Psychiatry.

There also has been discussion of changing nurses' hours to conform more to the hours of the school day. Nurses classically have worked a 7 A.M.-3 P.M. or 8-4 shift. If this could be altered to 9-5, child care would be simplified and more nurses would be able to work. Some of the obstacles to women's working come from traditional patterns of work hours

and locations, habits usually not at all essential to the tasks involved. Many of these could be and are being changed to adapt to the needs of working mothers.

Footnotes

¹Status is defined as a social position which is institutionalized, like mother, physician. As Goode points out "role" and "status" have often been used interchangeably (in "Norm Commitment and Conformity to Role Status Obligations," American Journal of Sociology 66 [November 1960], 246-258). We use the definition as given by Merton in outlining the categories (statuses) but will speak of role strain or role demands as used by Goode ("A Theory of Role Strain," American Sociological Review, pp. 483-496).

²Many of the responses follow the pattern that Merton has identified as the strain toward consistency. That is, where sectors of a social system are in conflict, there appear to be mechanisms at play which push for alleviation of the conflict.

³David Caplovitz describes this as the "person-set" (that complement of persons who may hold the same status) in "Student-Faculty Relations in Medical School: A Study in Professional Socialization," unpublished Ph.D. dissertation (Columbia University, 1961), Appendix A.

⁴Simon Yudkin and Anthea Homes in Working Mothers and their Children (London: Michael Joseph and Co., 1963), p. 54, show that mothers with one child are more apt to work than mothers who have two or more.

⁵This is akin to Goode's mechanism of compartmentalization. "A Theory of Role Strain," op cit, p. 486.

⁶Virginia MacMakin Collier, Marriage and Careers: A Study of One Hundred Women Who Are Wives, Mothers, Homemakers and Professional Women (New York: The Channel Bookshop, 1926), pp. 59, 74.

⁷Low and Spindler, Child Care Arrangements of Working Mothers in the United States; and Henry C. Zazewski, Child Care Arrangements of Full-time Working Mothers (Washington, D.C.: U.S. Department of Health, Education and Welfare, 1959), Children's Bureau Publication 387.

⁸Here we are using the mechanism of observability of the individual role activities, and observability of conflicting roles by members of the role-set mechanisms outlined by Merton to facilitate the individual's performance of roles. "The Role Set: Problems in Sociological Theory," British Journal of Sociology 8 (June 1957), 113 ff.

⁹Lotte Bailyn, "Notes on the Role of Choice in the Psychology of Professional Women," Daedalus 36 (Spring 1964), p. 708.

¹⁰Information from Kama Koslova (Soviet exchange student) at the Bryn Mawr conference on the Role and Status of Women in Soviet Russia.

¹¹Bailyn, op cit., pp. 708-709.

¹²Jesse Bernard, Academic Women (University Park, Pa.: Pennsylvania State University Press, 1964), p. 105.

¹³Talcott Parsons, in particular, see The Social System (Glencoe, Ill.: The Free Press, 1951).

¹⁴Philip Selznick, T.V.A. and the Grass Roots (Berkeley: University of California Press, 1953), reported the op-opting mechanism as used by a government agency: local leaders were asked to participate in a program, and thus gave support in a situation where they might have been obstructionists.

¹⁵Alice S. Rossi, "A Good Woman is Hard to Find," Transaction 2, No. 1 (November-December 1964), 20-23.

¹⁶Riesman, Glazer, and Denney, The Lonely Crowd, quoted in Alex Inkeles (ed.), Readings on Modern Sociology, p. 113.

¹⁷Introduction to Bernard, Academic Women, p. xx.

¹⁸Journal of the American Medical Association 196 (April 18, 1966), 128.

¹⁹Oswald Hall, "The Stages of a Medical Career," American Journal of Sociology 53 (March 1948), 327-336.

²⁰Ibid., p. 328.

²¹"Whatever incipient, still very shaky identity they had acquired as nurses was buttressed by family and hometown friends who, much to the student's surprise, now approached them as specially knowledgeable and wise in matters related to health." Fred Davis and Virginia L. Oleson, "Initiation into a Woman's Profession: Identity Problems in the Status Transition of Coed to Student Nurse," Sociometry 26 (March 1963), 97.

²²Elihu Katz and Paul F. Lazarsfeld, Personal Influence: The Part Played by People in the Flow of Mass Communication (Glencoe, Ill.: The Free Press, 1954), p. 7.

²³One cannot become a traveling salesman and then be surprised that work takes him from his family.

²⁴This is partially a function of the obligations becoming "visible" to others and thus is one of the mechanisms Merton outlines in "articulating" the role set in "The Role Set, Problems in Sociological Theory," British Journal of Sociology 8 (June 1957).

CAN WOMEN REALLY HAVE IT ALL? SHOULD WE?

by Letty Cottin Pogrebin

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ACTRESS COMBINES FAMILY AND CAREER WITHOUT TEARS.
ASTRONOMER THANKS LUCKY STARS SHE SEES KIDS ON WEEKENDS.
MOTHER OF FIVE MANAGES HOUSEHOLD AND STEEL COMPANY--AND
HUBBY APPROVES.

In the 1950s and 1960s, such stories promised exotica: how the glamorous career mom lives; can a lady executive be a real woman?; ten tips for the woman who wants both love and success.

Then, during the 1970s, as a confused reaction to women's new demands for autonomy and freedom of choice, those features changed from a few idiosyncratic human interest stories to inspirational literature for a new cult: Having It All.

The once-isolated profiles expanded into how-to books, special-interest magazines, conferences, and seminars run under some version of the same come-on: how to be superwoman, or having it all. For therapists, assertiveness gurus, and career counselors, a new clientele was born.

By now, it seems that every woman is supposed to want Everything. And Everything means the three-ball juggling act: job, marriage, children. The trick, they say, is to find the right formula; figure out a timetable that makes most efficient use of the crucial years (roughly age 20 to 35) and don't make the mistakes your mother made. She narrowed her sights. She gave up her own dreams for the family who sapped her energy and gave only a derivative glory in return. Only now is she discovering herself and her potential. She came last--and that's too late for you.

So you'll reverse the sequence and play the movie backward. Autonomy, but just for a while, before togetherness sets in. First, personal success. Become established in a career. Maybe 10 years later when you are self-confident and accomplished (these instructions rarely admit women's real problems in the job market), then get married. Add children to the scenario after there's enough money for live-in help, and before the biological clock strikes midnight.

Voila! You've got it all.

Unfortunately, it doesn't work that way, can't work that way, and maybe shouldn't work that way. Not as long as having "everything" is defined within the context of a culture where the men invent the feminine ideal and where women's needs are subordinated to men's economic and social convenience.

Several facts give away the lie. First, men are never expected to do the same juggling act. They still escape an equal responsibility for children, home, and keeping up a relationship. Second, in a patriarchy, having everything assumes that you start off with your basic stick-to-the-ribs wife/mother identity--and the rest is "liberated" gravy.

It means continuing to define woman's fulfillment by man's validation of her through his marrying her or promoting her in his workplace or, put another way, Everything means affirmation-by-approval of a member of the ruling caste.

It means adding roles--in the classic masculine dynamic of "more is better"--instead of changing or enriching the roles we already struggle to fulfill. It means measuring your stature and seriousness by the "masculine" embellishments you accrue--like career dedication or aggressiveness or making money.

If Having It All were really a new human definition, we would see men filling out the bones and sinews of their occupational success with the flesh and flood of "feminine" embellishments--like nurturing children, loving kindness, and staying home.

But of course, we do not. No, the juggling act is a gender-specific type: it is meant to occupy and silence the New Woman, that colossal complainer with her trouble-making Movement. And the New Man isn't standing in her way: Let's give her what she wants. Let her do it all. Why not? Sure she can rearrange the chronology, redesign her life plan, persuade herself that she's got it under control. As long as there are no tremors beneath the status quo; as long as men don't have to give up their political supremacy, good jobs, hot dinners, casual fatherhood, sexual services, birth-right power.

When a woman decides to have it all as is, nothing changes. Except the woman's expectations, her standards for herself, and her demands upon her already depleted energy. But the burden is on her. And institutions are undisturbed.

There's a nasty rumor that it's the fault of the Women's Movement for making women want Everything, an accusation about as sensible as blaming the Movement for male impotence. In fact, many feminists become feminists precisely because we have been trying to have it all. We discovered it is simply impossible. Without equal parenting and more societal responsibility for child care, without equal responsibilities and increased flexibility in the workplace and at home, there is virtually no person who can do it.

This is why feminists are not content to treat a social malignancy--like patriarchy and women's oppression--with a local anesthetic, like "have a baby, get a job, you'll feel better." There must be systematic political change.

We weren't the ones who created imperatives--like "you're not a real woman unless you're a mother with a full-time job." We called for options; we want the right to choose one or two of the balls without feeling guilt or failure if we decide not to juggle more.

Furthermore, we have discovered from each other's experiences that none of us has it all in society's eyes or in our own. The woman with a loving husband, three children, a successful career, an activist commitment, and a teaching job one night a week feels driven and compulsive and inadequate to the Everything she's supposed to be enjoying. The single woman with a career plan, free-flowing personal life, and economic self-sufficiency feels the Everything she has is diminished because she's only a one-ball juggler in a world that expects a three-ball circus act.

The single mother, the poor mother, the unemployed women, the older woman laboring in an unglamorous job with no time left for a "life plan"--where does she plug into the Everything ethic? It was never enough to be a single career woman. If it's now not enough to be a "good mother," or an honest, hard worker, or a neighborhood heroine, how much farther is her long march to economic and social parity?

For all Women, whose choices have always been limited by sexual caste prejudice as well as economics, the alternatives remain shortsighted personal solutions. We continue to want Everything, nonetheless, because it is distinctly human to strive for the unknown and because personal growth lies in the direction where we have never seen. The solution is not to stop wanting or to stop seeking a solution but to refuse to substitute one generation's imperatives with another's--to finally invent our own individual ideal of The Full Life, and our own means of getting there.

Before devising any blueprint that includes the assumption of Having It All, we need to ask ourselves these questions: Why do we need Everything? Must Everything include "family fulfillment"? Does "family" only mean having our own husband and children? Will it be so easy "to give up" the autonomy nourished during those first 10 years of self-cultivation? Are we asking change of anyone other than ourselves--like the men we live with and the institutions that govern our lives? (Would men be just as willing to limit their careers during childbearing years should that be necessary? And would the society accept them if they do?)

Most important perhaps, we must ask why it is that our struggle for survival in an unjust system is countered not with political change, but with upping the ante on female perfection.

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Career Options and Lifestyles, or What Do You Want to Be When You Grow Up?

There is a social myth that most people grow up, choose a job and a spouse, and live happily ever after. This myth contrasts sharply with reality. Most people have several major job changes during their lifetimes, many people either lose or change spouses or never marry, and relatively few people have work histories that progress in the orderly fashion associated with the term "career." Believing in this social myth of the perfect life has several consequences. When expectations about the future are unrealistic, disappointment and self-doubts are common. When occupational choices are not related to other lifestyle choices, there is a potential for conflict between them. And when these choices are seen as permanent, there is little motivation to learn the decision-making skills helpful in coping with changing situations throughout adult life. Developing these skills in the process of making an initial career choice will prove valuable in making later decisions.

In all decision making, the processes that are involved are: a) defining the problem, b) listing the alternatives, c) listing the values employed in judging the alternatives, d) evaluating, and e) acting. In the realm of occupational career choice, the definition of the problem and the alternatives are often influenced by social factors, such as whether the person is male or female, rich or poor, educated or uneducated, old or young. Possibilities for success are greatly influenced by the availability of employment and the accumulated skills and talents of the individual. Desires for wealth, independence, security, social relationships, excitement, or self-esteem influence how these options are evaluated. Occupational choice cannot be treated apart from choices about family life, leisure, and community service, in the development of a total lifestyle.

It is difficult for anyone to make lifestyle decisions based on personal preference, because we all have been bombarded from birth by other people's opinions of appropriate or possible lifestyles. Particularly for women, the virtue of serving the needs of others is often emphasized as an important "feminine" quality. When these "feminine" domestic qualities conflict with an expectation of career competence, as learned in school, a woman may find that the process of choosing a lifestyle based on her personal needs and wishes may become particularly uncomfortable, guilt-producing, and difficult.

Whether the final lifestyle chosen resembles that of many other women or whether the choice is unique, the personal satisfaction of creating a lifestyle for oneself is a basic right of every individual. The information included in this

section is designed to encourage and simplify the process of making that choice a personal one.

When one is ready to make a decision, there are many sources of aid. Information about one's personal characteristics that might relate to a particular career choice can be obtained from skills inventories and interest inventories given at local counseling centers. If these services are unavailable, careful introspection and list making can be helpful. Learning how to identify and clarify one's personal values and then applying those values to the choice of a lifestyle can be accomplished either by using one of the workbooks listed in the bibliography or by working with a career counselor. The biographies of women included in this collection, pamphlets from career information libraries, materials from the Women's Caucuses of professional societies, and discussions with other people are all sources of information about the range of career options and career characteristics. The selections on Work and Family in the preceding section provide information on possible consequences of integrating work with a family or choosing an independent lifestyle. The bibliographies included in this section provide sources of information to use in decision making and action. Specific career information or job projections have not been included, because current information is best obtained from placement and employment offices.

Because of the prevalence of sex-role stereotyping in our society, career guidance literature, testing, and counseling may be sex-biased. Materials useful for evaluating these services for sex bias are found in the bibliography. If the services and materials available are sexist, using one of the self-directed career workbooks may be preferable.

The articles in this section provide a brief introduction to career development of women as seen from the perspective of guidance counselors and sociologists. Also included are some examples of college women choosing a career. You may want to compare the ideal decision-making process outlined above with these descriptions of the way many women have behaved in the past, and the way you and your acquaintances are currently choosing your lifestyles. For example, consider the following questions:

1. What factors within myself will influence my career choice?
2. How has my career choice been influence by my family, friends, teachers, TV, and public opinion?
3. What are some differences in the processes used by men and women to make decisions about careers?

4. When did I first think about a career, and how have my ideas changed?
5. Does the career counseling that is locally available meet the needs of the female students? If it does not, how can it be changed?
6. Where are alternative sources of help in making decisions about a career?

Choosing a Career

"How Women Choose an Occupation" is part of a more extensive study of the ways by which college women choose a career. A sample of women students from an eastern technical college were questioned about their career plans every year for four years. The authors found that only 30% maintained their original choice or narrowed their occupational choice, whereas the remainder changed career plans. These changes, however, did not result in more consistency between career plans and work values or self-concept; instead they reflected movement toward typically feminine occupations. By comparing women who chose typical careers with those who chose atypical ones, the authors attempted to clarify the differences between the way women choose a career and the way men are thought to do so.

HOW WOMEN CHOOSE AN OCCUPATION

by Shirley Angrist and Elizabeth Almquist

Reprinted from Careers and Contingencies, Cambridge: Harvard University Press, 1975, pp. 135-144.

We lack understanding of the process by which a young woman finds and enters a work role reasonably congruent with her self-concept. Not everyone succeeds in making such a link before graduation. A few clues from the theoretical literature and a thorough analysis of the current findings show that women who choose atypical or male-dominated occupations do not differ appreciably in the process of making a choice, yet they do differ greatly in the content of the influences on their choice. Work values, occupational role models, and personal work experience comprise the major factors differentiating atypical from typical choosers. Let us consider these in closer detail.

We classified occupations as atypical for women in the following way. Women constituted at least one-third of the adult labor force, according to the 1960 Census. If they were distributed in the labor force in the same proportion as men, each occupation would have the same make-up by sex--approximately two-thirds male and one-third female. Obviously this was not the case. Hence, any occupation with less than one-third women was termed male-dominated, or atypical for women. All others were judged typical.

By this scheme, 34 women made masculine choices as seniors: lawyer, college professor, and biological, chemical, or psychological researcher are the most frequent atypical choices. Miscellaneous male-dominated selections include personnel management, economist, and statistician. High-school

teaching, social work, journalism, consumer economics, translator, dietician, executive secretary, and buyer were all classified as feminine or typical women's fields.

The results indicate an association between major field of study and the choice of male-dominated occupations (Table A). Students in the natural sciences are most likely to select an atypical occupation; those who do not are primarily future teachers or plan to work as laboratory technicians in the pharmaceutical industry. Women in the social sciences and the humanities are almost equally likely to choose female or male-dominated jobs. Majors in the applied fields are very unlikely to choose a masculine job, primarily because nearly all these students are in home economics or business studies, training to be secretaries, dieticians, or teachers.

Table A. Major Field and Atypicality of
Occupational Choice for Seniors

Major Field	N	Percent choosing male-dominated occupation
Natural Sciences	13	62
Humanities	20	50
Social Sciences	18	56
Applied Fields	<u>36</u>	<u>17</u>
Total	87	39%

The university experience apparently does not lure women into pioneering fields. There are no marked gains in the number of students preferring male-dominated occupations. The school's input shows up primarily through the opportunity for students to sample among various majors. Since these are bright, capable students who chose this professionally oriented school, they are rather receptive to various opportunities. Looking at the choices made all four years, 65 percent choose a male-dominated job at least once. But 85 percent indicate a traditional choice at least once. So sex-typing prevails, with women more likely to consider and eventually settle on feminine occupations rather than masculine ones.

Both typical and atypical choosers are about equally likely to postpone or change their occupational choice, and to feel uncertain about whether they will actually pursue it. The two groups do exhibit slightly different patterns of change. The typical choosers are about equally likely to exhibit no change at all, or to move from a detailed, specific choice to stating only a vague field of work. The women who choose male-dominated occupations more often follow the pattern of stating a general field and then narrowing to a specific occupation, or of changing choices within a particular field.

The work values students bring with them and maintain through the four years of school noticeably distinguish the masculine from the feminine choosers. Table B shows that there are only three work features on which the judgment of the two groups coincide. They both favor an occupation the can be combined with family life, and are only moderately interested in a stable, secure future. Both groups downgrade the notion of high prestige as an occupational feature. On the remaining items the students' preferences clearly diverge, with the typical choosers selecting the people-oriented aspects of work. Women who prefer male-dominated occupations more often prefer high income; they insist on being free from close supervision; and they are adamant about wanting to use their special abilities. Those who choose female occupations express the more traditional feminine values of working with people rather than things, helping others, and suiting their parents' ideas of success.

One further value separates the masculine from the feminine choosers. Students are unlikely to seek a male-dominated occupation unless they are also strongly career-oriented. Among the career aspirers, 59 percent choose male-dominated while 41 percent choose female-dominated fields. And of those women who are not career aspirers, a sizeable 78 percent choose typical women's occupations while only 22 percent seek atypical fields. Those few noncareerists who do select male-dominated occupations may be quite unrealistic. For example, one student said her choice is "to be a consultant and adviser in a large company in the fashion world, coordinating sales, advertising and market research." When asked if she knew anyone who now holds that sort of job she replied, "No, really it is

Table B. Atypicality of Occupational Choice and Work Values

Work feature	Atypical choosers	Typical choosers
	Percent quite or very important	Percent quite or very important
Allows use of special abilities	100	79
Allows combining career with good family life	71	73
Provides freedom from close supervision	71	58
Has prospects of high income	65	42
Provides stable, secure future	56	52
Occupation has high prestige	18	17
Involves work with people rather than alone or with things	62	81
Involves helping others	44	54
Suits parents' ideas of success	6	19

just something that I conjured up in my mind as the ideal situation for me." Perhaps such women mean that they are really not interested in working, but if they do, it will have to be a desirable and prestigious job.

The other "contrary" pattern is somewhat more understandable. It includes women interested in a career for themselves but who plan to work in a typically feminine field. About one in five students display this pattern. Few of these women plan to work while their children are very young, but they consider work an important part of their lives. When we asked one of these students if she wants to teach after having a family she replied:

As soon as I got them into school I would. Right now I can't see myself just making beds and doing dishes. But I know I like to teach seniors in high school. . . . I don't think I want more than two children because you can't support more than that well. As soon as they are six years old I will work because I just won't be satisfied sitting around the house. I have to be around people and it would add to the income so I could help in two ways.

Apparently working is seen by this student as a way to escape boredom and to earn money. Most of the career salient, typical choosers plan to be teachers or social workers. These choices are ones that the students believe can be combined with marriage. They also reflect a strong desire to serve society.

Work Experience and Occupational Choice

Slocum found in 1956 that high school and college women saw work experience as the single most frequent source of influence on the occupations they chose. Our data also plainly show that work experience affects students' occupational decisions in several important ways. Some of the students planning to be department store buyers decided against that option after serving an internship in that field. Others reported deciding against various types of work after looking more closely at the occupation. The students' limited exploration of various occupations through course work was also helpful in orienting them. Change in major frequently occurred after sampling the work possibilities via course content, although courses and term projects were secondary sources of occupational knowledge.

More important, the amount of work experience significantly differentiates between women who choose male-dominated occupations and those who select female-dominated occupations. Sixty-two percent of the former group had held four or more jobs compared with only 34 percent of the latter. Women who choose masculine fields also had a greater variety of work experience: 71 percent had held three or more different jobs. Only 38 percent of the typical choosers had worked at three separate positions. The greater amount and variety of work experience exposes students to a broader view of the composition of the labor force. Those who choose atypical fields must have a more thorough knowledge of the requirements

of various lines of work. Students who have more work experience perhaps can better appreciate their own competencies and preferences.

We wondered whether the actual jobs students experienced might be tied to the type of occupation they select. The atypical choosers are slightly more likely to have worked in jobs related to their senior occupational preference. The most frequent jobs students held were camp counselor, secretary, salesgirl, and assistant in father's business. Teaching, swimming, dancing, or music occurred often, as did library, hospital, and department-store work. By senior year some women had technical experience in fields connected with their college major, such as hospital and laboratory technician, technical writing, social work, and assisting with psychology experiments. Half the class members were privileged in this respect. Working in entry jobs similar to the one's preferred occupation is an important aid in clarifying occupational plans.

Work experience is crucial in helping students to decide on a particular line of work in another way. Sixty-nine percent of those students who were very certain they would actually pursue their chosen occupation had held four or more jobs. Less than half of the students who were fairly certain or undecided had that much work experience. Similarly, two-thirds of the women who were very decided had held one or more jobs related to their preferred occupation. Less than one-third of the students who had not yet chosen an occupation as seniors had any job related to their tentative occupational choice. The possibility for narrowing down a field of choice through work experience, and especially in a related field, seems to encourage choice of an atypical occupation. Or perhaps a mutual reinforcement occurs so that women attracted to male-dominated occupations show more initiative in testing out their interests through job experience. . . .

Who Influences the Atypical Chooser?

On each questionnaire students were asked to indicate from a list which person had most influenced their choice of occupation. If they were undecided about their occupational choice, that is, did not feel they had chosen an occupation, they were asked to indicate which person they expected to influence their choice. Responses were classified into three categories: (1) family, primarily mother or father, and husband; (2) occupational role models who were college teachers or persons in the occupation chosen; and (3) myself or no one. Women who choose male-dominated fields are twice as likely as the women who choose female-dominated jobs to say that occupational role models have been the major source of influence on their choice. Women choosing typical fields more often say that their parents, usually their mother, was the important influencer. The several undecided women who were considering feminine occupations tend to say that they expect their husband (or future husband) to heavily influence their choice.

The senior interviews amplify these findings. Many students could not give a detailed retrospective account of their occupational decision-making and we did not press hard for them to do so. What is striking

about their response is that the women choosing male-dominated fields could give richer descriptions of how they settled on a particular occupation than the other students. These women had more often been impressed by occupational role models; either college teachers had taken a personal interest in them and their work or they already knew persons in the occupation they planned to pursue. More importantly, they had received inputs from several sources rather than just one person. Consider the following exchange between the interviewer and a student who planned to do college teaching and research in chemistry:

Student: I guess my interest in chemistry originally stems from my father and my family. Our family is educationally oriented. My sister, my mother and my father are teachers. I was originally going to go for high-school teaching. But I felt that teaching high school would not allow for the more intricate aspects of chemistry, and I wanted to do research as well as teach. I decided to go more for college teaching.

Interviewer: Other than your family, do you know of other particular persons who might have influenced you?

Student: I would say my research advisor for the past two semesters. I consider him one of the few people who is really interested in other people. I guess in a way I am trying to copy him.

Interviewer: What would you want to copy about him?

Student: Just his interest in people. I feel that because of his vocation he can pursue both his interest in science and his interest in people, whereas out in industry it is a great deal more difficult.

Another student's comments illustrate the way in which those with atypical preferences frequently get to know their college professors as friends and work closely with them on a project. When asked what persons had influenced her choice of psychological research, she replied:

Dr. S. is really quite a guy. He is informal and never condescending and yet you know he knows so much more than you do. And I guess Bob M., who is temporary head of the computer department. I got to know him both on an academic and a social level. We talk a lot about computers and the fact that I am interested in that. And now the work I am doing with Dr. W. It turns out that the experiment I did for him last year on the way kids solve math problems is a perfect continuation of his research. He analyzed my data in relation to his data-processing model and it is fixed and we are publishing. All these big things are happening.

Occupational role models are also important because they aid the student in clarifying and realizing her plans. One student planning to do

biological research in industry was asked if she knew anyone already employed in that kind of work. Her reply:

Yes, a couple of my friends in biology who work in different companies. I just sit down and talk to them and become familiar with what people do. I learn about the attitudes of the companies, how they treat people. . . . One of my teachers had worked in industry before. He teaches you the very practical kind of stuff you will need to know, not just a classical education. When I get a job I won't have to say, I never learned any of this and you are going to have to teach me from the beginning.

The masculine choosers have more extensive contact with people outside their immediate family than do typical choosers. These contacts are sufficiently rare and unusual that some women feel dramatically influenced to enter that particular occupation. By contrast, the women who select female-dominated occupations more often feel influenced by persons within their immediate family. Their descriptions of the choice process are less enthusiastic and more matter-of-fact. A business major, asked how she decided to become a teacher, replied:

I don't know. I just kind of fell into it. I transferred out of history because I wanted something more practical. My mother has always wanted me to take the teaching option for the security of it. I thought, well it is not going to take too many electives, so I may as well go into education and I will have something behind me if I want to get married. Then I can always go back into teaching.

Although the women with typical occupations are generally less explicit about the routes to their particular job, they too sometimes feel aided by occupational role models:

Mrs. W. gave me the first organized impression I had of teaching and she gave me more facts. After I had her for a while, she seemed a very nice teacher and a very good teacher, and she would mention things like hours and the working conditions, and I guess it was part attitude and part facts that I got from her that made me decide to teach, too.

More often these students drift into a field because it is convenient or expected:

I guess you have to have something to fall back on and teaching is such a great profession for that. You can never tell what the future is going to hold for you. One reason why I went into teaching . . . my father always mentioned it, my step-mother and my mother and my step-father. Since they knew how fantastic I was with kids they kept pressuring me toward this. I decided that I really want to teach.

In this article, we have reviewed our findings about occupational choice and especially highlighted some differences between women who prefer male-dominated and those who prefer female-dominated fields. Although many in our study select masculine career fields, the majority do not. They slip quietly into traditional, feminine pursuits. In spite of the professional-vocational-masculine emphasis at this university, sex role predominates in the values women hold and in the occupations they choose. The general experience of college does little to aid women in extricating their occupational plans from sex-role concerns.

COUNSELING AND CAREER (SELF) DEVELOPMENT OF WOMEN

by L. Sunny Hansen

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Female Career Patterns

There is no full-blown theory of female career development. While most of the career development literature has dealt with male populations, Super (1957) postulated a "Logical Scheme" of women's career patterns. He identified seven kinds of female patterns, including what he labels the stable homemaking, the conventional, the stable working pattern, a double-track pattern, the interrupted pattern, the unstable pattern, and multiple trial pattern. The first attempt of any major theorist to direct his attention toward female participation in the world of work, Super's descriptive schema was prefaced with the somewhat prescriptive statement, "Woman's role as childbearer makes her the keystone of the home and, therefore, gives homemaking a central place in her career."

Others also have offered descriptions of women's patterns by socio-economic divisions. In 1968, for example, Psathas suggested the importance of cultural and situational factors and chance elements in the environment which limit women's freedom of choice. Anastasi (1969) identified the blue-collar pattern, the active volunteer, the interim job, the late-blooming career, and the double-life pattern. Zytowski (1969), like Super, began his "contribution toward a theory" with the assumption that the modal role of woman is homemaker. He then identified three factors which affect female vocational development: (1) age of entry into an occupation, (2) span of participation, and (3) degree of participation. Combinations of these elements yield three different vocational patterns which he labeled the mild vocational, the moderate vocational, and the "unusual" (the latter being the career-oriented woman).

These theories are important not because they provide the last word on women's career development but because they open the doors to research and provide some beginning attempts to understand women in other than traditional stereotypic roles. They also offer support for the thesis that women's life patterns are not uniform and that a variety of lifestyles and multiple roles are possible, desirable, and feasible for women as well as for men.

Sex-Role Socialization

There is another growing body of literature on women's growth and development that offers startling evidence of the limits on that growth. Although space does not allow detailed summary here, the studies of early sex-role socialization present convincing evidence of the programming of girls and boys for prescribed roles (Hochschild, 1973; Hartley, 1960; Maccoby, 1966; Weitzman et al., 1972). The textbook messages are clear:

boys are active, outdoor, strong, and breadwinners; girls are passive, dependent, weak, and homemakers; boys need to be able to be smart, to take care of themselves, boss, do a variety of jobs; girls are to stay behind, watch, wait, work puzzles, help boys, and stay home (New Jersey Commission on Women, 1972). In one book of phonics, the 21 consonants are boys, the 5 vowels are girls. The girls all have something wrong with them; when put with the consonants, the girls lose their names and their identities. One book about girls shows a dejected little girl sitting on the steps asking, almost plaintively, "What can I do?" The parallel book about boys shows a standing, active, happy boy saying, "What I can do." Girls grow up narcissistic, asking "How will I look? What will I wear?" Boys learn early that they can be, as one preschool book for four- to six-year-olds suggests,

A pirate, a sailor, a gypsy, a knight,
An actor, a cowboy, a king.
I'll be strong, it shouldn't take long,
I'll be five by spring.

That this programming is reinforced in early childhood became evident recently at my daughter's sixth birthday party. Having bought a variety of inexpensive role-free gifts for the children to fish out of an imaginary pond, I let the children fish randomly for such toys as a police set, doctor set, binoculars, jump ropes, dental set, nurse set, etc. While the game was in progress, I was surprised to find one of the girls in tears. When asked what was the matter, she replied that she had gotten a dental set and that was for boys; she wanted jewelry instead. It was interesting, however, that one of the other girls, whose father was a dentist, also had randomly fished out a dental set; she was pleased because her father wanted her to become a dentist (she was one of three daughters).

Obviously, we are not trying to force children to make choices regarding what they want to be at the tender age of six; we do want them to fantasize and develop their sexual identity (as different from traditional sex roles). However, such examples are significant because, as studies such as those by Goodman and Schlossbert (1972) have shown, children link occupations with sexes and begin the premature occupational foreclosure process early in life.

Female Self-Concepts and Aspirations

What does this early conditioning do to female self-concepts and aspirations? Matthews and Tiedeman (1964) found that girls who had expressed strong vocational goals in junior high had shifted to marriage goals in senior high, although the more recent study by Rand and Miller (1972) suggested that a new cultural imperative for women was being expressed in the options perceived by girls in junior high, senior high, and college--that of the dual role of career and marriage. Other studies have cited women's lack of vocational goals and realistic planning (Lewis, 1965; Zytowski, 1969) though more recent studies, such as the follow-up study on Project Talent population, reveal girls as showing more concern for career planning and wanting more control over their own lives (Flanagan and Jung, 1971).

Horner's widely quoted study of female career motivation (1968), though recently replicated with different results (Hoffman, 1974), does give cause for reflection. Academically talented college females, asked to complete a story about "Anne, who was graduating at the top of her medical class," revealed all kinds of "fear of success" themes, fantasies that it couldn't be true--having Anne drop to eighth in the class and marry the boy at the top; seeing her as an acne-faced bookworm experiencing feelings of rejection, loneliness, and doubts about her femininity; having her see a counselor who suggests that she try nursing; and the like. The young women simply could not cope with the image of Anne as a competent, feminine person who might be able to have a successful career and marriage. In another oft-quoted study of Broverman et al. (1970), mental health practitioners were asked to describe a mature, well-adjusted man, woman, and person. The descriptions for the well-adjusted person and well-adjusted man coincided. However, the well-adjusted woman was described as more submissive, less independent, less adventurous, more easily influenced, less aggressive, less competitive, more emotional, excitable, and vain, and less interested in science and mathematics. While the results of self-concept studies are conflicting, some of the investigations of female self-concepts have found that girls tend to devalue themselves and other girls and that both boys and girls value males more than females. These studies seem to indicate a number of factors which mitigate against women feeling very good about themselves as achieving, motivated, participating human beings.

Women in the Work Force

What happens when we move from the theoretical descriptions of women's lives and the sociological and psychological studies of their self-perceptions and aspirations to the realities of their participation in the work world? Even here the data can be somewhat shocking, especially to the high school boy or girl who may find it difficult to internalize the information. While there are numerous myths about working women, there is an abundance of data available on the nature and extent of women's participation in education and work--data gathered by occupational analysts in the U.S. Department of Labor.

Women and Employment

It is well-known, for example, that there are some 32 million working women, comprising one-third of the labor force; that 42% of all women are working, over half of them married; that most women work for economic reasons; that the number of employed mothers, even those with small children, has increased and that there is an increasing proportion of female heads of households.

We are told that the average woman marries at 20 (although some women appear to be marrying later and having fewer children), has her last child at age 26, her last child in school by 32. The average age of women in the labor force is now 42. With a life expectancy of 74 or 75, the average woman can expect to have 30-35 years after children (if she has them) are in school to develop new meaning and interests in the

second half of her life. A chart prepared by the California Advisory Commission on the Status of Women presents very vividly a typical married woman's life (Figure 1). Of course there is considerable variation in work force figures, dependent on whether the woman is from a minority or poverty family, her marital status, the number of children she has, and her work motivation. But the long-range Labor Department projections are that 9 of 10 girls will marry; 8 of 10 will have children; 9 of 10 will be employed outside the home for some period of time; 6 of 10 will be widowed before age 50; 1 of 10 will be heads of families; probably 30 of 10 will be divorced; and 1 of 5 will obtain a college degree (Impact, 1972).

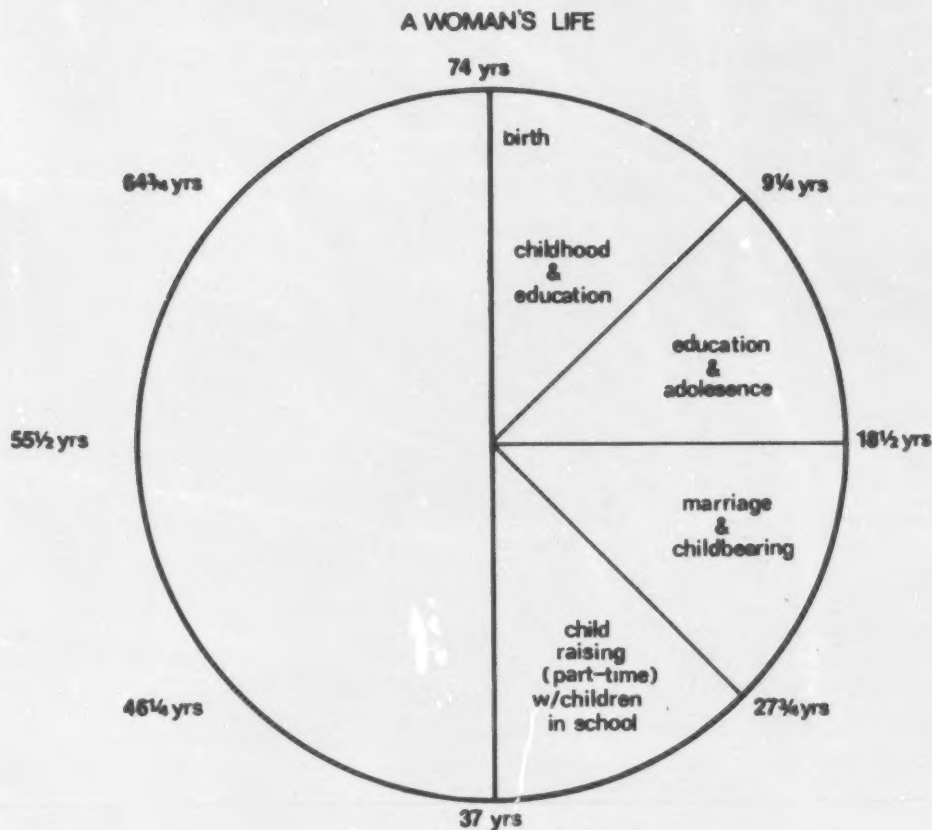


Figure 1

The blank half could be filled with "vegetation," employment, political activity, community work, church or club work, etc., but should it not be filled with productivity and the utilization of talents, abilities, and interests? Do people stagnate if they don't continue to grow? What happens to a woman who hasn't worked in 20 years if she is suddenly widowed or divorced at age 43? Shouldn't the full-time homemaker's child-raising years be devoted part-time to continued education and preparation for the last half of her life? (Prepared by California Advisory Commission on the Status of Women, 1972.)

Occupational Distribution

One of the hard realities is that women who are working are concentrated in a few occupations; many of them low-paying, low-level and dead-end. Although the Dictionary of Occupational Titles has classified approximately 23,000 different occupations in the United States, one-third of all working women are concentrated in only seven of them: retail sales clerk, secretary, household worker, elementary school teacher, book-keeper, waitress, and nurse. An additional one-third are found in 20 occupations: e.g., typist, cashier, cook, telephone operator, baby sitter, assembler, hairdresser, stenographer, high school teacher, practical nurse, receptionist, maid, and file clerk (Bem and Bem, 1971). Only four million women--15% of all women workers--are professional or technical workers: women comprise only 7% of the physicians, 3% of the lawyers, 1% of the engineers. The proportion of women in professional jobs has declined over the past 30 years, from 45% in 1940 to 37% in 1969. Three of four clerical workers are women. The average full-time female worker makes three-fifths of the earnings of the full-time male worker in all occupational levels and fields. A more detailed presentation of these statistics appears in Kreps' (1971) Sex in the Marketplace: American Women at Work and Kievit et al. (1972) Women in the World of Work as well as the numerous pamphlets published by the Labor Department. Such are the realities of women and work.

Obstacles to the Career Development of Women

It must be apparent that the obstacles to the development of women are real and pervasive. While they have been alluded to, the most common ones are summarized below:

1. Sex-Role Conditioning and Socialization. If it is true, as appears increasingly to be so, that on many of the major variables in human development (self-concept, moral development, career aspirations, peer evaluation) female development levels off in early adolescence, counselors need to be justly concerned. The image females get of themselves through our curriculum and child-rearing practices seems to have taken hold, the self-fulfilling prophecy comes true, and 78% of women end up in the same role despite their individual differences.

2. Role Conflicts about Fulfilling Multiple Roles in Marriage and Work. While there is a natural expectation that men will be able to fill multiple roles of employee, husband, and father, we do not have that same expectation for women. Thus the woman who is considering both career and family may experience role conflict. Besides the fear of success and concern about femininity which Horner (1968) found, the woman may be caught between trying to vie with men in jobs, career, and business and at the same time trying to find an identity as wife, mother, and woman. She may face the problem of meeting the multiple demands on her once she has accepted multiple roles and may find she has to be a superwoman to meet those demands--to organize her time, to manage the household, to attend to her children, to have an abundance of energy--unless she has a partner who recognizes that such roles and household tasks can be

shared. If she has small children, she may be criticized for not spending enough time with them--although the literature on the employed mother suggests that her children are no less well-adjusted than other children, that they are more independent, that they have more career aspirations and, when asked to name the person they would most like to be like, most frequently mention their mother (Bem and Bem, 1971). Increasingly, it must be recognized that women's development is both a male and female concern, for when women increase their participation in work and community the lives of men and families will be affected. The problems of the re-entry woman with children, for example, may include the way in which her husband and children cope with her transition from a traditional to contemporary woman. Problems associated with the changing roles and status of women require not only (1) the resocialization of men's attitudes towards women's roles, as Farrell suggests, but (2) the resocialization of women's attitudes towards men's roles and (3) of women's attitudes towards women's roles. As we move toward an androgynous society, we may see a greater variety of life patterns--the equal partnership marriage, in which both partners have professional careers and share that part of their career that is in the home; the two-person career, in which one job or appointment is shared by two partners who want to work 20 hours or so and have more time for other parts of their lives; the extended family, in which members develop alternative life patterns for work and family and leisure; or the single parent who prefers not to marry but adopts a child. One of the ways we can reduce the role conflicts is to be more accepting of a variety of life patterns and life styles.

3. Focus on Marriage or Its Prospect. We know that the modal role is still marriage, in spite of increasing choices of the single life. But we need to make our young men and women aware of the fact that life does not end at 40 and that Prince Charming is not going to take care of his Princess forever. The modern fairy tale of "Atalanta" in Free to Be You and Me provides an excellent antidote to this myth. Young people need to be made aware of those last 25 to 35 years of a woman's life and to do some conscious planning for their preferred lifestyle. Consistently studies have shown that girls lack planfulness, that they tend not to seek occupational information, that they lack realistic educational-occupational plans. If it is true that both boys and girls "do not know what information they need, do not have what they want, and cannot use what they have," we have a responsibility to help them get this information, to use it and to internalize it in terms of their own goals, plans, abilities, preferred lifestyles, and self-images.

4. Lack of Work Orientation. While junior high girls and upward are beginning to see themselves in multiple roles, especially dual roles of career and marriage, women simply are not as work oriented as men, nor are they expected to be. Working outside the home in the past has not been as central to women as to men, and those who have career motives at the head of their motivational hierarchy are labeled "unusual" as Zytowski (1969) suggests. Concern for women's career development is not a movement to get every woman into the labor force but, rather, a concern for her uniqueness and individuality as a person and for her right to have some freedom of choice in both her personal and work life. It is concern

about the overwhelmingly subordinate nature of women's roles--as nurses rather than doctors, teachers rather than principals, assembly workers rather than supervisors, secretaries rather than bosses, bank tellers rather than lending officers, administrative assistants rather than deans. It is concern about the ancillary nature of women's careers, with only small numbers in banking, engineering, medicine, and management. It is concern about the passivity and dependence that keep her from finding room at the top even if she has ability. It is concern about fear of competency that keeps her from maximizing her potentials and from making what Tyler (1972) has called first-class rather than second-class contributions to society. It is concern about the complexity of demands, pressures, and conflicts facing women at different life stages and the limited reward system which denies them the range of options and rewards available to men. What we need to do here has to do with changing self-concepts and expectations, opening up opportunities, and upgrading aspirations so that more talented women will be able to say not "What can I do?" but "What I can do."

5. Sexism and Sex Discrimination. While it is true that some of the barriers between women's work and men's work are being broken down, the discrimination in hiring, wages, and promotion is still very real, affirmative action programs notwithstanding. The problems of conscious and nonconscious sexism (Bem and Bem, 1971) are as prevalent in education as in other parts of society. All of us need to look at ways in which we perpetuate discrimination and sexism through our actions, inactions, and even denials that a problem exists.

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The theme of the following article is the practical importance of responsiveness to current trends in the labor market. The data and projections change from year to year, however, and comparison of these data with more recent projections by the Labor Department indicates the importance of continually seeking current information to use in career planning.

WOMEN, CAREERS AND COUNSELING: THE NEW ERA

by John B. Parrish

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What Are the Characteristics of the New Labor Market of the 1970s?

Is there really a new kind of labor market for college women in the 1970s and beyond? There is. It represents a sharp break with the past. The Carnegie Commission on Higher Education was quite right, after an exhaustive study, in giving one of its recent summary reports the title: College Graduates and Jobs: Adjusting to a New Labor Market Situation. We shall note here eight characteristics of this new labor market.

First, the outlook for teaching is changing. About half of all college women until recently have been employed in the teaching profession. It will not be possible for teaching to absorb this number in the future. In 1972, a record 337,619 men and women completed college teaching preparation programs. (1) They made approximately 2.3 million applications for 179,000 teacher vacancies. About 176,000 were hired. This left a surplus of 162,000 or 48% of the graduates. (2) Although the total number of graduates in teacher training appears now to be declining, demand is also declining, leaving a gap that could continue and even worsen in the coming years. Within this teacher surplus environment, women are losing out to men in their share of teaching positions. In 1930, women held 65% of all secondary teacher positions. By 1971, this had fallen to 46%. (3) The competitive disadvantage of women is intensified by their lower levels of educational attainment. Whereas 50% of male teachers had completed five years of college or more in 1973, only 27% of the females had this much college training. (4)

Second, replacing teaching as a career choice for college women is not going to be easy. In many respects elementary and secondary teaching has been a unique occupation for college women. It was the largest, most respectable white collar professional occupation for women. College educated mothers just expected their daughters to enter teaching. Many daughters felt very comfortable in this stereotyped role. Teaching was ubiquitous. Teaching jobs were available everywhere. No matter where a husband might move, a school system of some kind was nearby. Teaching had natural affinity with family life. A woman could teach a few years and transfer her experience into raising her own children. Then she could return to teaching other people's children later, having benefited from experiences with her own children. Teaching fitted in well with family vacations. There are few alternative occupations that provide the unique traditional features of teaching. Many college women may believe they are being forced to give up a great deal in redirecting career choices away from teaching. And they could be right. Redirection won't be easy.

Third, making the right career choice is very much more important for college women today than in the past. The choice will become even more important in the future. This is because college women are working more and more of their post-college lives. A special analysis of the 1960 Census of Population indicates that 96% of college women worked at some time after college. (5) The percentage is virtually 100% today. Another study reported that college-educated mothers are likely to work at least half of their post-school lives and that college-educated single women work nearly 100% of their lives to retirement. (6) In March 1973, nearly 60% of all women with four or more years of college were in the labor force. If they had five years or more of college, 70% were in the work force. In the early post-college years, 20 to 24 years of age, nearly 90% were working. Even during the primary childbearing years, ages 25 to 34, 60% were at work if they had four years of college and 75 percent were at work if they had five years or more of college training. (7) The work lives of college women are moving very rapidly toward closure with that of college men. A generation or so ago, when women, college and non-college, worked only a few years and then left the labor market for good, a mistake in career choice was not too serious. Now that college women are working a larger percentage of their post-school lives, it can be very serious.

Fourth, women are facing tougher competition from men. This statement is true for several reasons. For one, men have been obtaining more years of college training than women. In 1952, there was little difference in the percentage of men and women completing four years of college or more--7.7% for women and 8.1% for men. By 1973, a gap had been created favoring men, with 12.0% of women completing four or more years of college and 15.6% of men in this same category. (8) Another reason the statement is true is that the federal equal rights legislation and administration are having an impact. Equal job opportunities work both ways. Women are now entering jobs formerly restricted to men, but men are now entering occupations formerly dominated by women, such as elementary teaching, nursing, secretarial fields, or serving as stewardesses.

Fifth, college women will face much more competition from other women in the future. Not too far back in time, a college degree had scarcity value. This value is declining rapidly. In 1957-1959, 54% of women in the labor force had less than a high school diploma. This gave the 19% with some college training a decided advantage in competing for jobs. By 1970-1972, 36% of women workers had less than a high school diploma and 27% had some college training. It is projected that by 1990 only 19% of working women will have less than a high school diploma and 36% will have some college training. (9) The former educational pyramid is being turned upside down. The unique competitive advantage of college training is disappearing. This will reinforce the point, made above, that a mistake in career choice in the past was not too serious, but that it will be much more serious in the future.

Sixth, college women from now on will face a plus and minus market with wide variations in supply and demand relationships. It has been estimated that overall demand for college graduates in the 1960s was about 8.0 million. Supply was only 5.5 million, leaving a shortage of 2.5 million. College students didn't have to seek jobs; the jobs came looking for students. (10) In the 1970s, it is estimated that, under the most favorable conditions, supply and demand for college graduates will be in rough balance at around 9 million. (11) This overall balance, however, is misleading. As illustrated in Table 1, there will be some occupations in which the supply is likely to exceed demand by an extreme margin. For example, the annual supply of students majoring in history is estimated at 47,000. Annual openings will be around 800. Similarly, political science will have an estimated annual supply of 26,500. The demand will be around 800. Many women in such fields will face extreme disappointment in job hunting.

There will be other occupations in which estimated demand will exceed supply. Accountancy is a good example, with annual supply being around 21,000 and openings numbering 33,000. Women majoring in this field should do extremely well.

For some occupations, supply and demand will be in approximate balance. For example, it is estimated the annual supply of lawyers will be around 15,600; annual demand will be around 13,000.

These are only estimates. Supply-demand relationships can change. The raw materials and energy shortages have already raised the demand for engineers above earlier estimates. But so far the estimates in general have proven accurate enough to indicate that college women may no longer drift through college taking interesting courses and be assured of a satisfying job on graduation. The favorable market for college graduates in the 1960s is gone and is not likely to return. More careful career search and counseling from now on will be necessary.

Table 1
Selected Occupations with Three
Types of Projected Supply-Demand
Relationships, 1968-1980

Supply Will Exceed Demand			
Profession	Projected Supply	Annual Openings	Annual Excess of Demand Over Supply
Anthropologists	3,677	200	3,477
Economists	19,609	2,200	17,409
Historians	47,036	800	46,236
Life Sciences	34,102	15,200	18,902
Newspaper Reporters	6,084	1,800	4,204
Political Scientists	26,495	800	25,695
Demand Will Exceed Supply			
Profession	Projected Supply	Annual Openings	Annual Excess of Demand Over Supply
Accountants	21,405	33,000	11,595
Dentists	3,408	4,900	1,492
Dieticians	1,458	2,700	1,242
Medical Laboratory Technicians	1,830	12,800	10,970
Oceanographers	279	400	121
Optometrists	463	800	337
Physicians	8,025	20,000	11,975
Statisticians, Actuaries	944	1,900	956
Supply and Demand Will Be in Approximate Balance			
Profession	Projected Supply	Annual Openings	
Biochemists	1,087	700	
Chemists	15,620	12,800	
Lawyers	17,871	14,500	
Librarians	6,949	8,200	
Engineers			
With Engineering Degree	40,000	73,000	
Entering Engineering Without Degree	35,000		
Pharmacists	4,379	4,400	
Podiatrists	204	200	

Source: United States Department of Labor, Occupational Manpower and Training Needs, Bulletin 1701, 1971.

Seventh, the rate of change in the occupational structure is likely to accelerate in the future. This means counselors' knowledge of occupations is likely to become obsolete at an accelerating pace. Old traditional jobs may decline rapidly. New jobs may emerge rapidly. This quick change can be illustrated in the case of the health occupations. When the first edition of the Health Careers Guidebook was issued in 1955, it listed 150 health occupations. (12) When the second edition was issued in 1965, the volume described 206 health occupations. (13) When the third edition is issued in 1975, it is expected to describe at least 270 health occupations. In recent years the American Medical Association has approved an average of one new type of paramedical training a year. Unless counselors engage in continuous study, they are apt quickly to become uninformed and misinformed.

Eighth, the task of destroying occupational stereotypes about women's careers held by students, parents, and counselors is a very large and difficult one. The nation's largest single profession is engineering. About 1,300,000 persons are currently employed in this field. Women have long demonstrated their abilities to make excellent, top-flight engineers. The professionals are well paid. Engineering practice, marriage, and children are compatible. In the light of the nation's raw materials and energy needs, demand for engineers has been rising. At the same time, supply has been declining. Since the peak of 43,000 engineering graduates in 1971, the annual graduating class declined to 42,000 in 1974 and is expected to reach a low of 34,500 in 1978. (14) Women graduating in engineering have had their choice of jobs at entrance salaries somewhat above men. This favorable outlook should continue through the 1970s. How many women students, parents, and counselors are aware of this favorable demand-supply relationship? Even if aware, how many can break away from the old stereotype that "engineering is not for women"? Apparently not many. Surveys of career choices by incoming freshman college women report that in 1966 only one-half of one percent said engineering was their career choice. In the fall of 1973, only one percent said engineering was their choice. (15)

Are College Women Adapting?

Are college women adapting to this new labor market and changing career choices? An overview answer may be obtained from Table 2. In June 1972 nearly half of all women earning baccalaureate degrees obtained them in education. And this accounted for the surplus noted earlier. A national poll in January 1974 reported only 32% of all college women said teaching was their number one career choice. Among entering college women, Fall 1973, only 21% said teaching was their career choice. It appears that counselors, parents, and students have done very well in reducing the tendency of women to concentrate on teaching. The rise of women's interest in business and health fields may also be considered a favorable trend. It would be a mistake, however, to conclude all is well in women's higher career choices. First, there remains a much higher concentration of women's interests in just two fields, teaching and health, in contrast to the greater occupational dispersion of men. Secondly, it

Table 2
Percent Distribution of College Students
by Sex and Field

Baccalaureate Degrees, June 1972
Career Goals, January 1974
Career Choices, Freshmen, Fall 1973

	1		2		3	
	All U.S. Baccalaureate Degrees June 1972		Career Goals of All College Students January 1974		Career Choices of All Entering College Freshmen Fall 1973	
FIELD	PERCENT DISTRIBUTION					
	Men	Women	Men	Women	Men	Women
Total All Fields	100	100*	100*	100	100	100*
Education	11	46	5	21	17	32
Business and Commerce	24	3	19	8	17	13
Science and Mathematics	24	7	19	11	9	7
Engineering			13	1	7	**
Social Science	20	17	13	12	4	9
Humanities	13	18	8	16	5	9
Health, Medical	2	7			10	20
All Other Fields	6	3	19	25	17	5
Undecided	-	-	5	5	7	6

*Total Exceeds 100 Percent Due to Multiple Response

**Less Than One Percent

Source: Data in column 1 from United States Department of Labor, Educational Attainment of Workers, March 1973. Special Labor Force Report 161. Data in column 2 from Gallup Poll, January 1974 as reported in Scientific Manpower Commission, Scientific, Engineering, Technical Manpower Comments, June 1974, 11, 18. Data in column 3 from College Placement Council, Update, May 1974, I, 5.

may be observed that women's interest in two of the most promising areas, the biological and physical sciences and engineering, remains extremely low. Yet these are areas of expanding employment opportunities. The message has gotten across about restricted opportunities in teaching. It hasn't gotten across about promising opportunities in the sciences and engineering. Further, nearly three of ten entering college women in Fall of 1973 chose the social sciences and humanities as career goals. Four-year degrees in these two fields are not likely to be rewarding in terms of careers.

The Move into Elite Professions

That talented women are beginning to make more commitments to long-term, high-level careers other than teaching is shown by recent trends in professional training. Table 3 presents the evidence for eight selected fields.

The need to meet the new challenge is urgent. There is very real danger that colleges and universities could graduate a rising number of disappointed, disillusioned, unhappy, talented women, not because rewarding career opportunities for them will not exist, but because they will be unaware of their existence.

Table 3

Women as Percent of Total Enrollment
Eight Professional Fields, 1960, 1972.

Field	Percent Women of Total Enrollment	
	1960	1972
Architecture	5	6*
Dentistry	1	3
Engineering	1	3
Law	4	12
Medicine	6	13
Optometry	1**	5
Pharmacy	12	25
Veterinary Medicine	4	14

*1971

**Estimated

Source: Parrish, J. B., "Women in professional training."
Monthly Labor Review, 1974, 97, 41-43.

Footnotes

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11. United States Department of Labor, Occupational Manpower and Training Needs. Bulletin 1701, 1971.
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15. College Placement Council, various surveys as compiled in Update I, 5.

MORE UP-TO-DATE PROJECTIONS

COMPARISONS OF ANNUAL GRADUATES AND ANNUAL OPENINGS
1974-1985

	Annual Graduates B.S./B.A.	Advanced	Annual Openings
Engineers	56,700	20,000	73,000
Geologists	4,054	1,168	1,900
Geophysicists	84	103	650
Meteorologists	294	249	200
Oceanographers	237	269	100
Life Scientists	68,400	15,430	16,400
Foresters	2,480	551	950
Range Managers	163	62	150
Mathematicians	19,205	5,871	1,550
Statisticians	257	600	1,450
Chemists	10,064	3,583	10,000
Astronomers	152	159	30
Physicists	3,625	2,308	3,000
Anthropologists	8,396	1,940	250
Economists	13,972	2,932	4,700
Geographers	5,202	1,069	650
Pol. Scientists	32,760	3,420	600
Psychologists	69,045	11,698	5,200
Sociologists	37,150	3,675	750
E/S Technicians	42,408*		32,000

*Vocational Education Completions

SOURCE: Bureau of Statistics

Reprinted from Betty Vetter, Supply and Demand for Scientists
Engineers. A Review of Selected Studies. Washington DC:
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Resources for Making a Career Choice

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CHECKLISTS FOR EVALUATING SEXISM IN COUNSELING

Career Quotient Checklist¹

<u>Generally True</u>	<u>Generally False</u>	
_____	_____	1. Since 1960 increased numbers of women have entered employment, thus the proportion of women in the labor force has increased.
_____	_____	2. Women have a higher turnover and absenteeism rate than men.
_____	_____	3. Teaching has been, and continues to be, a good occupation for women to enter.
_____	_____	4. Predictions are that future workers will be better educated than workers in today's labor force.
_____	_____	5. It is risky to hire and train a woman, because most women are employed only a short time of their lives.
_____	_____	6. A majority of women who work do so out of economic need.
_____	_____	7. Housewives are happy women.
_____	_____	8. It's risky to hire a married woman, because she's not likely to remain on the job long.
_____	_____	9. It is estimated that 80% of America's working people are not working to their full capacity.
_____	_____	10. Most people prefer not to have a woman supervisor.
_____	_____	11. A graduate with a "B" average and work experience is more employable than an "A" student without work experience.

¹ Items for the Career Quotient Checklist were adapted from Birk and Tanney (1972), Bolles (1972), and Figler (1975).

- _____ 12. Many jobs are more appropriate for women rather than for men, and vice versa.
- _____ 13. Studies show that the employment of mothers leads to juvenile delinquency.

Correct Answers for the CQ

1. True.
2. False. In 1968 turnover rates in manufacturing for men and women were 4.4 and 5.3, and 4.4 and 5.2 respectively. Numerous studies have found that turnover and absenteeism are more related to the level of the job than to sex. There is a higher turnover rate in less rewarding jobs, regardless of sex of the employees.
3. False. During the 1950s and 1960s when school enrollments were rising rapidly, large numbers of college graduates entered teaching. Through the mid 1980s, however, school enrollments are expected to level off while new college graduates are increasing; therefore, the proportion of new college graduates who will be able to obtain teaching jobs will decline.
4. True.
5. False. Nine out of 10 young women will spend 25 or more years working. The more education a woman has, the more likely she is to work.
6. True.
7. False. Married women are 60 times more likely to become clinically depressed than single women.
8. False. Sixty percent of working women are married; one-third of all mothers are employed.
9. True.
10. False. In one study where at least three-fourths of both the male and female respondents (all executives) had worked with women managers, their evaluation of women in management was favorable. On the other hand, the study showed a traditional/ cultural bias among those who reacted unfavorably to women as managers.
In another survey in which 41% of the firms indicated they hired women executives, none rated their performance as unsatisfactory; 50% rated them adequate; 42% rated them the same as their predecessors; and 8% rated them better than their predecessors.

11. True.
12. False. Jobs, with extremely rare exceptions, are sexless. Women are found in all of the 479 occupations listed in the 1960 census. Tradition rather than job content has led to labeling certain jobs as women's and others as men's. For example, although few women work as engineers, studies show that two-thirds as many girls as boys have an aptitude for this kind of work.
13. False. On the contrary, research indicates that many factors must be considered when seeking the causes of juvenile delinquency. Whether a mother is employed does not appear to be a determining factor.

Checklist for Sex-Fair Career Development Materials¹

	<u>YES</u>	<u>NO</u>
1. Do the illustrations show men in traditional masculine careers and/or women in traditional feminine careers?	—	—
2. Do the illustrations show women as helpers predominantly, and/or men as the leaders or authority?	—	—
3. Do the illustrations show women mainly in passive postures (such as watching, sitting, or waiting) and/or men in active postures?	—	—
4. Do the illustrations tend to show women as smiling and pleasant, in contrast to men who are shown as serious and thoughtful?	—	—
5. In careers traditionally considered as "masculine," is the female figure shown less frequently than the male figure?	—	—
6. Is the generic "he" used for traditional male occupations, and/or the generic "she" for traditional female occupations?	—	—
7. Are women encouraged to consider traditional female jobs and/or men encouraged to consider traditional male jobs?	—	—
8. Are sex-based occupational titles used; for example, "fireman" rather than "firefighter," or "mailman" rather than "postal worker"?	—	—

¹Items 6 through 11 are adapted from Women on Words and Images, Help Wanted: Sexism in Career Educational Materials. New York: Educational Products Information Exchange, 1975.

- | | <u>YES</u> | <u>NO</u> |
|--|------------|-----------|
| 9. Is there evidence of tokenism? For example, one or two striking examples of equal sex treatment may be presented, but the material overall may be dominated by male role models and/or sexist language. | — | — |
| 10. If references exist to family responsibility, are there sexist expectations? Is it assumed, for example, that it is the woman's basic responsibility to raise and care for a family, and it is the man's basic responsibility to economically provide for wife and children? | — | — |
| 11. Are there references to appearance, physical qualities, or sex-based personality traits as being significant factors related to job success? | — | — |
| 12. Is there evidence of sexist language, such as using "man" or "mankind" rather than "people" or "persons," and using forms of the masculine pronoun (he, his, etc.) to refer to people in general? | — | — |
| 13. If the setting of the illustrations is outdoors, are men predominantly featured in the outdoor setting? | — | — |

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5. American Chemical Society
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6. American Council on Education
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Harvard University, Cambridge, MA 02138 617/495-3734
12. American Physiological Society
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15. American Public Health Association, Inc.
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16. American Society for Cell Biology
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U.S. Government. Federal and State Aid Programs. Superintendent of Documents, U.S. Government Printing Office, Washington, D.C. 20402. Stock #052-071-00179-4.

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- How and Where to Get Scholarships and Loans (2nd ed.) 1967 pap
by Juvenal Angel (\$3.50)
Regents Publishing Co. hb (\$6.50)
Division of Simon & Schuster
1 West 39th St.
New York, NY 10018
- Lovejoy's Scholarship Guide hb (\$4.95)
by Clarence Lovejoy, rev. 1964 paper (\$2.95)
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- More Education, More Opportunity (no charge)
U.S. Office of Education
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- National Catalog of Financial Aids for Higher Education (\$8.75)
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by Louis T. and Joyce W. Scaring

Library of Congress Catalog Card Number 73-16513

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<u>Addresses</u>	<u>Special Features</u>
Adolph Van Pelt Memorial Scholarship c/o Miss Olga C. Patterson Fargo Lane Irvington, NY 10533	For American Indian descendants
American Association of University Women Fellowships Program 2401 Virginia Avenue, N.W. Washington, D.C. 20037	Dissertation fellowships
American Indian Scholarships, Inc. 211 Sierra Drive, S.E. Albuquerque, N.M. 87108	
Bureau of Indian Affairs Higher Education Program P.O. Box 1788 Albuquerque, N.M. 87103	
Business and Professional Women's Foundation 2012 Massachusetts Avenue, N.W. Washington, D.C. 20036	Women over 30
California State Scholarship and Loan Commission California State Graduate Fellowship 1410 Fifth Street Sacramento, CA 95814	California residents
Carnegie Corportion of New York 437 Madison Avenue New York, N.Y. 10022	
Council of Southern Universities The Diuguid Fellowship Program Executive Director 795 Peachtree Street, N.E., Atlanta, GA 30308	Dissertation fellowships for older Southern women
Council on International Educational Exchange 777 United Nations Plaza New York, N.Y. 10017	

Danforth Foundation
222 South Central Avenue
St. Louis, MO 63105

East-West Center
Graduate Degree Study Awards
1777 East-West Road
Honolulu, Hawaii 96822

Educational Testing Service
Box 200
Berkeley, CA 94704

Educational Testing Service
Graduate Fellowships for Puerto Ricans
Box 2822
Princeton, N.J. 08540

The Ford Foundation
Graduate Fellowships for Native
Americans, Mexican Americans,
and Puerto Rican Students
320 East 43rd Street
New York, NY 20017

U.S. Department of Health, Education
and Welfare
400 Sixth Street, S.W.
Washington, D.C. 20201

College Work Study Program

Guaranteed Student Loan Program

National Direct Student Loan Program (NDSL)

Institute for International Education
809 United Nations Plaza
New York, N.Y. 10017

National Fellowships Fund
795 Peachtree Street, N.E.
Atlanta, GA 30308

National Research Council
National Science Foundation/
Graduate Fellowships
Fellowship Office
2101 Constitution Avenue, N.W.
Washington, D.C. 20418

Multidisciplinary problems

Mexican Americans and
American Indian
descendants

See Financial Aid Officer
in Local Institutions

Predoctoral Study Overseas

Social Sciences Graduate Study
for Blacks

489-492

Prognosis

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Prognosis: Women, Science, and Society

A major theme woven throughout this book is the diverse interactions between science and society. We have seen that working as a scientist provides an opportunity for some individuals to obtain financially rewarding employment while contributing their skills to society. Scientists, technologists, and professionals in science-based fields provide products and services to individuals and groups. They also provide information and opinions that may influence the views of many others in society. Conversely, culturally derived ideals, expectations and beliefs strongly influence the attitudes and behavior of scientists.

Until recently most scientists have been men, and traditionally masculine values and behaviors have dominated the image and performance of a professional scientist. A woman entering a male-dominated profession has had to accommodate in some ways to the demands of the profession as well as to the responsibilities of other roles she may choose to play. Since the number of women in most scientific professions is low, the individual woman has little influence on the profession; instead she is required to change to fit its masculine expectations. As the number of women entering scientific professions increases, women may be able to retain some of their constructively different ideals and attitudes even though they differ from the attitudes typical of the profession. In doing so, they may alter the behavior expected of scientists: they may change the profession.

Thus becoming a woman scientist has personal consequences for the individual and potentially widespread consequences for science and society. To illustrate this point we conclude with the following article discussing the consequences of increasing the number of feminists in one scientific profession, medicine.

CAN WE BE FEMINIST PHYSICIANS? MIRAGES, DILEMMAS AND TRAPS

by Mary Howell

Reprinted from Journal of Health Politics, Policy and Law 2 (1977):
168-172.

There are some mirages, dilemmas and traps in the confrontation of feminism and medicine, those two powerful and competing ideologies. My own working definition of feminism has three parts. It begins where it must, in an identification with all women as sisters. Women share some indignities, some deprivations, imposed not on the basis of our individual qualities, but because we are women. Much more important is the wealth of our shared experience as women; we have been fortunate in being better able than men to keep enduring ties to the dailiness of life. This identification with womanliness is the very obverse of elitism or privilege. Feminism, I think, leads more truly the opposite way, to gut-felt empathy with anyone who suffers indignation and deprivations.

Contrast this to the educational, training and work experience of physicians. We can hardly avoid the elitist trappings of privilege; these are assumed to be our due for being so smart, for working so hard, for playing God's games.

The second part of my feminism is an appreciation and enjoyment of caretaking. By caretaking I do not mean that mindless dirty work that women do because we have no better job opportunities, but the graceful, gracious humanity of caring for another who is in need or pain. It is anything but mindless; it is called "dirty work" only because our society does not acknowledge that caring for another human is a valued or valuable business. Society has shunted caretaking to women, but if we are feminists we know better than to accept the judgment that it is a less worthy occupation than designing automobiles, writing books, or making laws.

Contrast this, again, to the professed ideology of today's physician. It is the physician who heals, not the patient. "Handholding" is for nurses; doctors are too busy being scientists to have time for caretaking. Our training denies us our caretaking role.

The third part of my feminism comes from what I consider to be the privilege of women's place. Because we traditionally have lived in the center of our families, we know about mutuality of control; we are experienced in collaboration and we understand the difference between control and collaboration. To be controlled by another, as a chronic condition, is to be weakened, angered and ultimately diminished. To be in control of others, as a chronic condition, is to be self-aggrandized, insensitive, and, I believe, ultimately diminished in one's capacity to assist the healing process.

Contrast, once more, this view with that of the contemporary physician. We are expected to control our coworkers, presumably because "someone must be in charge in a life-and-death emergency." But few physicians work on a day-to-day or hour-to-hour basis with life-and-death emergencies. Most control their coworkers simply because that is the physician's privilege and habit. We also control our patients, presumably because they are not capable of understanding the informational grounds on which we make their decisions for them. We are not prepared or encouraged to teach patients what they must know to make their own growth- and health-promoting decisions.

On these issues feminism and medicine face off squarely. I believe that one reason for the elitist privilege, the avoidance of caretaking, and for the arrogation of control observable in the education, training and practice of physicians is that medicine predominantly reflects the traditional male values. Until the last few years, women who "made it" in this male profession were often hard-pressed to become "honorary men."

Let me point out, in the form of questions, some of the mirages, dilemmas and traps for women who hope to reconcile feminism and doctoring; you will have confronted others.

1. Should a feminist physician focus special effort on bettering health care for women? Unless we undertake that responsibility, no one is very likely to do so. In gynecology, obstetrics, pediatrics and internal medicine, stereotyped and demeaning assumptions about women blatantly interfere with the promotion of good health. And yet:

The entrenched attitudes of physicians in these fields have established conventions of care that are extraordinarily difficult to alter.

Residency for a lone feminist is even more gruelling than for other young doctors; and since there are so few women residents, let alone feminist residents, a feminist cannot expect to have a supportive group of colleagues during the critical years of post-graduate training.

Women live with men and in families. Perhaps the best improvement for women would be some alternative kind of care for themselves and for the others in their lives--changes in health care for everyone.

Medical care now is not deficient in healing potential for women only. Can one in good conscience devote a career to a separatist system of care? Who will look after the welfare of all patients?

2. Can we afford to work in bureaucratic settings? If you define a bureaucracy as I do, then only private solo practice, some very small groups, and collective or collaborative work arrangements are non-bureaucratic. It seems clear that a bureaucracy is a poor arrangement for the giving of human service and anathema to caretaking. Yet it is likely that a combination of legislation and third-party payment regulations will

soon make it almost impossible to work as a doctor except in a bureaucratic setting. It is extraordinarily difficult to "invent" a different kind of medical care with in a managed bureaucracy. On the other hand:

Some argue that bureaucracies can be humanized, but that they will not be unless we who hope to be a different kind of doctor try to work within them.

It is also argued that most patients will continue to have to seek help for their health problems through bureaucracies. "Alternative" medical services generally have been accessible only to small and special groups of layfolk. The poor, the unrepresented and the lonely usually must resort to the most rigid, intransigent and uncaring of bureaucratic clinics.

3. If caretaking is nearly precluded by the conventions of doctoring, ought we to find work in other niches of the medical world? We can do a great deal for our younger sisters, looking ahead to their own work lives, and for the image that women as a group hold of our competence and productivity, by being extraordinarily good researchers, administrators, teachers, policy planners. Yet if we do so:

Actual medical caretaking will increasingly fall to low status, low-paid health workers already locked into the dead-end jobs thought suitable for women.

Patients will continue to be instructed in dependency and incompetence.

4. Can we in fact avoid the privileges that go with physician status? Our salaries are disproportionately high compared with those of our equally competent and hardworking sisters who are nurses, technicians and secretaries. It is expected that we will allow others to pick up for us and do our errands. We get preferential treatment on waitin lists, in parking assignments, in giving our opinions on almost any matter. And yet:

Our work--especially if it involves direct patient care--is demanding. Can we justifiably risk neglecting either our patients or our families by refusing to accept privileges?

We must often expend real energy to refuse or avoid privileges that come our way quite "naturally" in our present social setting. Are we justified in deflecting that energy from the care of our patients, our important research, our attempts to influence public policy?

5. Perhaps most poignant, how are we to work with our sisters? The overwhelming majority of health-care workers are women; only a few are doctors in a position to decide the manner in which we will work. We who are physicians can easily fit our assigned slots and get along in the usual doctor/subordinate work relationships. Secretaries, nurses, technicians and assistants--almost all of them women--will pick up the loose bits of patient care for us, carry out our orders, cover our lapses, do our routine, boring and dirty work, wait on us. If

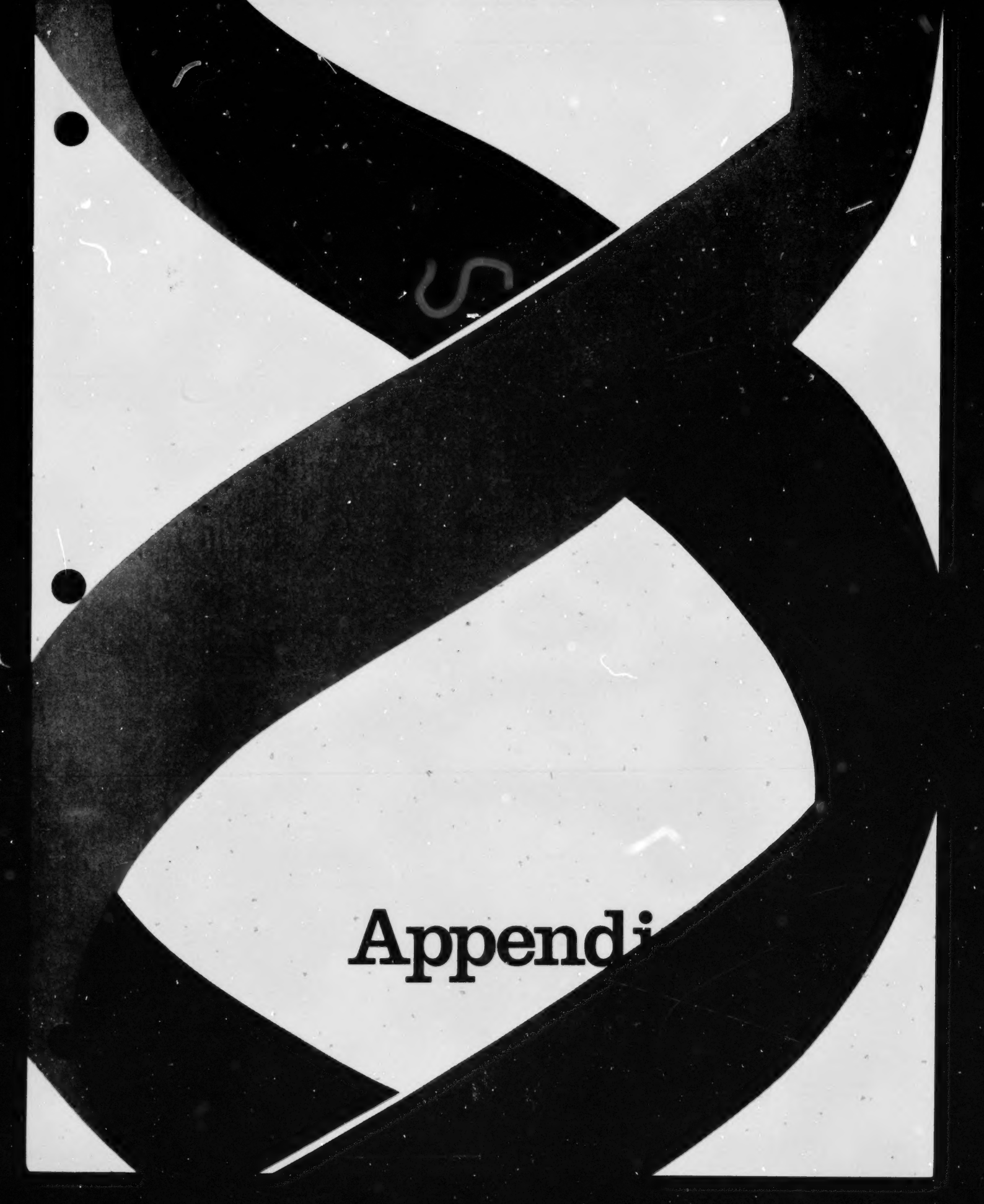
you have ever been a secretary, for example, you know that one of the worst features of the job is always having to do someone else's work and almost never being able to generate your own.

For us to learn how to work with women coworkers as collaborates in caretaking will require a painful process of challenging, unlearning, relearning, struggling. Both physicians and nurses are thoroughly trained in customary modes of relating to each other. I agree that we should work together. I would even argue that we physicians might well use our access to privilege to support the unionization of women health workers, the establishment of at-work day-care centers, and the expansion of the work roles of nonphysician caretakers. I have no disagreement, only full acknowledgement of how hard it is to change our habits and expectations.

6. One last, difficult trap: the newly expanded roles of so-called "allied" health workers seem to be designed on the assumption that the physician will be primarily a noncaretaking scientist-technician. Must we accept that role in order to protect the new and hopeful status of our sisters? In the extreme, is actual day-to-day primary-care no longer a job for feminist physicians, lest we encroach on sisters who do not need us beating them back? The distinctions between doctor and nurse, in both function and style, have always echoed the male/female stereotypes. But we must realize the threat that we sometimes appear to be to our nurse sisters.

These are perplexing matters. I am convinced that there are no single right answers; yet I also believe it is worthwhile to spend time and energy talking out such questions. Our decisions in the end are always a mix of what we as individuals want to do, what we can do, what we need to do, and the direction we can find from conscience and the caring support of friends. One last reflection out of feminist ideology; implicit in the identification with all women as our sisters is a grand tolerance for individual struggles, compromises and choices. I can better understand my sisters whose choices are different from mine if I remember that we all come from the same struggle.

I believe that the next few years may bring some striking changes in the roles played by women in health--not only because of the tiny foothold we have gained through Affirmative Action programs, or the growing strength of the women's health movement, or the increasing concern of women scientists with matters relating to women and health, or the blossoming awareness of lay-women of their potential for competence and self-reliance in maintaining the health of themselves and their families. In nearly every medical school there is now a strong cohort of women, many somewhat older than medical students traditionally are, more experienced in life, less easily brainwashed into docile acceptance of the contemporary conventions of medicine. They have an impressive potential strength. With each other for support, and the wider women's movement for reference, they may be able to begin to push medical care toward caretaking for health.



Appendi

Appendix A: Notes to the Instructor

The appendices of this book are designed to allow scientists to conduct a "Women in Science Program" that may be outside their field of expertise and to do so with minimal strain on their own careers. The following suggestions for implementation are based on our teaching experience during the two years this book has been in preparation.

We have found that organizing a program to encourage women to become scientists is an exciting and frustrating adventure. The excitement has come from sharing the rewards of our own careers with students and other faculty as we developed a new, supportive group of women scientists. The frustration has come from realizing that this activity has not been a highly regarded use of our already overcommitted time and is only a small contribution to the enormous task yet to be accomplished.

Our most important suggestion is to search out and utilize the talents of all of the women scientists and supporters of women in science who can be found. Although this search may seem initially time-consuming, there are many potential long-term rewards. Scientific colleagues can reduce the individual instructor's strain by sharing the work-load of coordinating an interdisciplinary program. This collaboration will also generate a larger group that can work to enhance the position of scientific women in the institution (for a particularly cogent outline of what needs to be done within any institution and within a discipline, see American Economic Review, vol. 63: 1049-1061). Gathering such a support group of friends of women in science increases the probability that many students will receive publicity about the program. It also provides an accessible, varied pool of mentors and role models, who are important because students seem to find contact with women scientists particularly valuable.

Coordination with nonscientists is also important in reaching students who may benefit from a "Women in Science Program." Helpful administrators can provide lists of women faculty and graduate students, information about students with high science and math scores, and assistance with school-wide coordination of an interdisciplinary program. Although we did not feel particularly experienced in writing the necessary press releases, producing descriptive brochures, or counseling students about their specific career problems, we did find reporters, advisors, and career counselors who gave essential assistance. Close cooperation with advisors was especially necessary for reaching our particular target of the incoming freshman or sophomore student who has not yet selected a major.

Because it is often necessary and usually advisable for scientists themselves to become involved in career education as they teach, we have made special note of articles helpful for noncounselors (in the bibliography on career counseling, in Appendix C). Familiarity with this literature enlarged our understanding of the need for our program and helped us develop an ease in advising students and in interacting with counselors. Depending on the local situation, it may also be advisable to share the bibliography in Appendix C with your local counselor, since many counselors have had limited or biased experience in counseling women interested in nontraditional careers.

We have included additional bibliographies on general references and sources of further information (Appendix B), and teaching aids, including audiovisual materials and program evaluation instruments (Appendix D). Although the bibliographies are not comprehensive, we believe they provide a useful introduction to the extensive literature on these topics.

As this book is designed to encourage students to risk attempting a new scientific role, so through these appendices we hope to encourage you, as scientists, to risk trying a new teaching role.

Appendix B: Further Information

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SOURCES OF FUNDS

Finding Funds for Programs Relating to Women's Educational Equity. (A general overview of the process of obtaining funds from foundations and government agencies, references to sources for funding.) Available from Superintendent of Documents, GPO, Washington, D. C. 20402. Order No. 017-080-01835-7. Cost: \$2.50 prepaid.

Fund for the Improvement of Post-Secondary Education. U.S. Office of Education, 400 Maryland Ave., SW, Rm 3139, Washington, D. C. 20202. (Possible source of funding for women in science projects.)

National Science Foundation, Directorate of Science Education, Washington, D. C. 20550. (Source of funds for, and information about, women in science programs.)

Women's Educational Equity Act Program, Women's Program Staff, Office of Education, 400 Maryland Ave. SW, Rm 3121, Washington, D. C. 20202. (Source of funding for women in science projects and information on other projects.)

Women's Educational Equity Communications Network. Far West Laboratory for Educational Research and Development, 1855 Folsom Street, San Francisco, California 94103. (an information service)

Appendix C: Career Counseling Information

Because career education is not customarily a part of science curricula, we include here an introductory portion of L. Sunny Hansen's article that specifically addresses people working with women choosing careers. Her more general discussion of the process of career choice by women can be found in Section IV. Her resource list and our bibliography that follow should provide a sound background to the scientist or counselor who is not familiar with this process.

COUNSELING AND CAREER (SELF) DEVELOPMENT OF WOMEN

by L. Sunny Hansen

Excerpted from Focus on Guidance (December 1974), pp. 1, 6-14.

Counselors today need to be concerned about the career development of women as well as the career development of men. For, as we are well aware, women do not exist in a vacuum or make decisions in a vacuum; what women do and can do affects and is affected by the perceptions and actions of men. By the same token, the limitations put on female development also create obstacles and prescriptions for the way men act, behave, and grow. Yet, it seems appropriate that special focus on the career planning and counseling process for women is justified at this point in time, though perhaps it will not be necessary in another 12 or 15 years. And perhaps, as human liberationists have pointed out, both men and women eventually will have more options open to them as a result of the current attention to the concerns and development of girls and women.

It is an underlying assumption of this article that both men and women can have greater control over their lives and development if both males and females, counselors and teachers all accept some responsibility for doing something about women's untapped resources, talents, and potentials. It would seem logical that counselors, who are supposed to be facilitators of human growth and development, should be key persons in bringing about changes in this area, in public school, college, and vocational school settings, not only through counseling but also through curriculum interventions and consultation with teachers. . . .

Practical Approaches to Facilitate Female Career Development

One of the things we know from organizational change literature is that it is futile to offer solutions before people recognize there is a problem. Unfortunately, to many parents and educators in higher education as well as K-12 settings, concern about women's career development is still considered a

"ha-ha." It is an assumption of this paper that counselors view it as more than a "ha-ha."

Counselor Attitudes

Counselors have been indicted in the professional literature as being sexist. While we need many more data on this (and I do not assume that counselors are any more or less sexist than people in general), several studies have supported these charges. Thomas and Stewart (1971) found that secondary school counselors responded more positively to female clients with traditional (feminine) goals than to female clients with deviant (masculine) goals. Pietrofesa and Schlossberg (1973) found differences in counselor attitudes toward women entering "masculine" occupations. Friedersdorf (1973) examined attitudes of male and female secondary counselors toward college-bound and noncollege-bound girls and found males with more traditional expectations about female occupational choices. Bingham and House (1973a) found counselors to be misinformed on women's occupational status (on such issues as women in the work force, discrimination against women, income discrepancies, and the probability of women getting leadership jobs). Male counselors were found to be less well informed than female counselors (on such issues as the occupational alternatives needed by women, their general ability, women's ability to fill both worker and mother roles, and length of time in labor force). In a follow-up study on counselor attitudes toward women and work, the investigators (1973b) found that substantial numbers of male counselors had negative attitudes and agreed with such statements as "training women for high-level jobs is wasteful," "married men should receive more pay than single women doing the same work," and "boys should be better educated than girls." They also felt that motherhood is the primary function of women. Hawley's study (1972), in contrast, suggested that female counselors hold a wider view of the roles of women. Such studies point up the need for counselors to become aware of their own attitudes and practices and ways in which these limit options for female clients.

Counseling Interventions

While the role of the counselor in school and college is changing in certain ways, it is likely that one-to-one counseling and group counseling procedures will still persist. Counselors can play a critical part (but not the only part) in counseling both boys and girls and men and women for changing roles in society. Matthews et al. (1972) have addressed themselves to the importance of counseling girls and women over the life span, from infancy through mature adulthood and old age. Tyler (1972) stressed the need for all individuals at various life stages to have counseling available regarding their multipotentialities. Below are suggested a few counseling strategies that might help make a positive difference in the lives of females.

1. We need to become aware of our own conscious and nonconscious attitudes, expectations, and practices in the counseling interview.

There a number of subtle and not-so-subtle things we do both verbally and nonverbally in our counseling that communicate to boys and girls what is acceptable and what is not. In our interviews, how do we react when a

girl says she wants to become a pilot, an engineer, or an auto mechanic? How do we help girls plan and choose their school courses and programs with open options? By the same token, how do we react when a boy says he wants to be a nurse? How much do our own sex-role stereotypes and expectations affect our counseling behavior? Among the things we might do to counteract our biases is to help females think of themselves as persons, to affirm their conflicts, to learn to say "I can." We need to be aware of the development stage the woman is at in her life span and "where she is coming from" as a person and as a woman.

2. We need to become increasingly aware of sex bias in guidance materials, tests, and inventories, and our own bias in interpreting and using those materials.

Schlossberg and Goodman (1972) have called attention to the channeling that can occur through use of biased interest inventories. The National Institute of Education (NIE) sponsored a national conference on this topic, resulting in a manual of "Guidelines for Assessment of Sex Bias and Sex Fairness in Career Interest Inventories" (NIE, 1974). It seems to me counselors have to be equally sensitive to other kinds of instruments, especially in career planning and exploration. A recent study by Birk, Cooper and Tanney (1973) of illustrations in an array of career literature--including the Occupational Outlook Handbook, the Encyclopedia of Careers and Vocational Guidance, Volumes I and II, 1972, and the Science Research Associates Occupational Briefs, 1973--revealed the same kinds of racial and sexual stereotypes that have been found in children's textbooks and picture books: women and minorities appeared only infrequently in comparison with white males and often were in passive, helping, subordinate roles. If there is one major area in which counselors have been criticized, it is in our selection, use, and interpretation of tests, earlier with minorities and more recently with women; we would be well advised to examine our own test interpretation practices and to help eliminate from use those that are clearly biased in the way they present opportunities for men and women in the world of work.

3. We need to know and help clients obtain accurate information about trends both in the world of work and in the larger society.

We have to do a better job not only of getting accurate information ourselves but of helping boys and girls get the information they need and to use it. They need information about themselves, the labor force (present and projected), work environments, alternatives, and decision processes. Because of this expectation of marriage and the Prince Charming myth, many girls do not seek education-occupational information.

Presumably if one is seeking a job merely as a stopgap until marriage, one does not need much information. When examining the human life cycle of woman today and the second half of her life, ask what kind of information does she need? When, how, and under what circumstances do we get across the reality of what is happening to the woman in American society--that she may be working 25-35 years out of her life even if she marries and takes time out for childrearing? And how do boys obtain and react to that same kind of information? How do we help him and her to internalize the infor-

mation? One way might be to provide group guidance and group counseling experiences in which boys and girls can talk together about these trends and what they mean in relation to both male and female goals and roles and family patterns and the possible androgynous society of the future.

4. We need to help young men and women become increasingly aware of the options available to them in a pluralistic society--in education, occupation, lifestyles, and career patterns.

We may have to help both boys and girls think through and plan for the multiple roles they may have as workers and parents. They need to be aware of the variety of lifestyles and family and work patterns from which they can choose and of potential conflicts involved in choosing one pattern over another (e.g. single life, multiple children, two-person career, dual career without children, etc.). Particularly, we need to help female clients consider a wide range of educational and occupational options in addition to the traditional stereotypic ones. This becomes extremely important at a time when the traditional options, such as teaching, for example, are becoming less available. Society is beginning to realize that college is not the only road to success and that other excellent opportunities exist in such institutions as vocational-technical schools. With the small percentages of women in the skilled trades, we should encourage adolescent girls to explore new and emerging training programs and occupations and expose them to contact with women in atypical fields. While we need competent secretaries, teachers, and nurses (of both sexes), we do not want women programmed into limited types, numbers, or levels of occupations; we want to help female clients choose from the larger pool of alternatives those appropriate to their abilities, goals, interests, and motivations.

5. We need to help young men and women learn the processes involved in decision making.

If counselors really are concerned about human development, they have to help each individual, regardless of race, sex, or age, to know that she/he can choose in accord with his or her values, abilities, motivations, and preferences from a variety of life options. Clients need to be helped to explore the alternatives, the probability and possibility of achieving the alternatives, and the consequences of choices they have for both themselves and the significant other in their lives. They need to be able to examine themselves as risk-takers, to critically evaluate the information about self and options, and to synthesize or integrate it as they think about themselves in relation to society. They need to be encouraged to challenge traditional assumptions and expectancies about roles and to realize that changing women's opportunities for different life patterns also has implications for work, family, and leisure patterns of men.

The problems of decision making for women have been cited often in the professional literature--the lack of planning orientation; the fear of loss of femininity if she chooses a career; the shift from vocational goals to marriage goals from junior to senior high; the assumption of marriage as the modal role and not perceiving the dual roles of marriage and work (or other patterns) as viable options. All of these contribute to the view that females do not

have choices or decisions to make, that they do not have alternatives to choose from since they are programmed from early childhood for the one option of marriage and motherhood. If females are to develop their potentials to reduce the gap between what they do and what they can do, counselors must help them know and be able to choose and decide from diverse life patterns, from traditional nuclear family to single parent to single person to dichotomous or multiple roles; they need to be helped to know that they can have goals of their own and an identity of their own.

6. We need to provide female clients with a variety of role models with whom they can identify and from whom they can learn that multiple roles are possible, desirable, and real.

The importance of putting females in touch with women who are in nontraditional roles cannot be overemphasized. Many of the early and late adolescents we counsel today are familiar only with the female image to which they have been exposed at home, in their school books, and in the media. Counselors can help broaden their views and expose them to wider options by helping identify women in the school and community who have chosen all kinds of career and family patterns.

7. We need to involve parents more systematically and developmentally in the career development process of boys and girls.

Since parents still have the greatest impact on their children's self-concepts, goals, attitudes, and aspirations, it is exceedingly important they be oriented to the facts about the life span of women, changes in the labor force, the need for career planning for girls, and the trends in work and family. Orientation and information groups for parents regarding the development of women's potentials might be one vehicle; perhaps what we need is a career development counterpart to parent effectiveness training called CDTP, Career Development Training for Parents.

These are just a few strategies we might use as counselors to become facilitators not only of female career development but of the development of all human beings for a variety of life roles.

Curriculum Interventions

The foregoing suggest modifications of traditional counselor roles in interviewing, in test interpretation, in career information, and in parent counseling. The other major thrust toward which counselors are moving today is a more central and direct involvement with curriculum, particularly through working with teachers in new ways. There seems to be a lot of support at every level of the educational process for counselors to move in the direction of outreach programs, to become a part of the mainstream of the teaching-learning process, and to take active leadership in changing the school system to more effectively promote the positive growth and development of students. . . .

At the senior high levels, we need to help girls and boys continue their values clarification and examine their needs, drives, goals, interests,

and abilities as they face real decisions about lifestyle preferences and life patterns. They need information about the reality of discrimination, trends in the work force, stereotypes, and sexism. Apprentice or shadowing experiences with a variety of role models in preferred occupations and tryout tasks will help them reality-test their preferences and tentative decisions. They need more specific information about educational paths and exposure to vocational specialties or college majors related to subjects in which they have a continuing interest and success. Direct courses in Psychology of Self, Psychology of Interpersonal Relations, and Psychology of Careers as well as Women's Studies can facilitate such development. Life Planning Labs in which students have an intensive opportunity to examine values, potentials, goals, and priorities can also be helpful, as could a variety of well planned consciousness-raising and role reversal exercises. Strategies summarized by the author include infusion through curriculum, exploratory work experience (paid and unpaid), career resource centers and multimedia approaches, hands-on experiences, role models, counseling, cross-age teaching, and staff development. . . .

There has been a burgeoning of activity to promote female development at the post-high level, particularly on college and university campuses. Besides the traditional counseling these interventions have taken such forms as women's resource centers; courses on assertive training, career planning, and women's identity; personal assessment and career planning groups; courses for the mature or adult student, women's support groups (counseling groups, job-seeking groups, feminist groups, human sexuality groups), and special seminars and conferences, women's study programs, creation of alternate study options, and the like; research topics related to women's development; units on sexism in human relations courses; creation of multimedia presentations and video cassettes for training of counselors; and creating and evaluating intervention models.

The Task for Counselors

It is probably safe to say that we have just begun to chip away at the top of the iceberg of the enormous problems and implications of counseling and career development of girls and women. The topic, like the larger career development area itself, is still unfinished business; there is much we need to know about female career patterns, self-concepts, aspirations, and decisions. And yet we know enough to chart some humanistic paths which will lead to greater options and genuine freedom of choice for both men and women. The following poems express the essence of my concern--the first a negative example, the second a positive one.

The following is a song students are asked to learn in a career education program in a junior high school, sung to the tune "Jingle Bells":

Styling hair, styling hair
To make you gals look neat,
So that hubbys when at home
Will see their wives look neat.
Fixing twirls, fixing swirls,
Maybe a French bob,
Don't let feminine society
Look like crumbly slob.

Besides being bad poetry, this is not career education but career miseducation. This is not what we are about as teachers and counselors involved in career development programs. In contrast, the following poem appeared a few years ago in an elementary level career development project which offered a variety of methods to help children gain more positive self-concepts, to upgrade their aspirations, to feel good about themselves. The poem appeared on the cover of the project booklet.

DISCOVERY: A CHILD'S FIRST AWARENESS OF HERSELF

She looks into the mirror with eager eyes,
And all the world is bells, and she is wise,
The wonder of herself she sees therein.
And longs to play the world's violin.

Her name is written and no turning tide
Will wash it from the sand or oceans wide.
She feeds on knowledge and her mind is stirred,
Fed on the beauty of a thought, a word.

Where she must walk, a slant of light has shown,
Knowledge is the lamp her heart has known.
And when she thinks of all her eyes might find,
She says, "Quickly, pull the cord and lift the blind."

Adapted from Kaleen Sherman

This poem, adapted from Kaleen Sherman, is a beautiful expression of the openness to life, to knowledge, to growth that a child feels. And yet we do something to females in our society as they grow up, something that keeps the blinds drawn on many of their possibilities and potentialities.

When I say "adapted," I should hasten to explain that the poem originally was written using "he." But isn't it equally beautiful and equally meaningful with "she"? It is exciting to think what we as counselors could do, even with some slight changes, a few facts, some innovative approaches, and a commitment to open the blinds of the school and eliminate our own biases to promote the positive growth and career development of our female clients along with our male clients.

Resources for Counselors to Use in Promoting Women's Development

A variety of media, materials, and resources has been developed to help counselors and teachers begin to deal with the special concerns of women. Besides the usual printed resources, there are numerous games, simulation exercises, questionnaires, and films, cassettes, and filmstrips which can be selected to meet diverse objectives. While they cannot all be catalogued here, a few of them are described below.

Handbooks and Guides

Sexism in Education, a handbook developed in 1973 by the Emma Willard Task Force on Education, offers a variety of activities for teachers and counselors to look at their own attitudes, their instructional materials, and their classroom procedures, as well as suggesting role playing and consciousness-raising techniques. Available from Emma Willard Task Force, University Station, Box 14229, Minneapolis, Minnesota 55414.

Sex-Role Sterotyping: Implications for the Human Services, published by the Minnesota Resource Center for Social Work Education, 721 21st Avenue South, Minneapolis, Minnesota 55404, contains a wealth of information and exercises on employment, education, law, and social/personal life, with implications for childrearing, employment, education, and politics. A special section deals with human services practices and provides three case examples for role playing.

How to Go to Work When Your Husband Is Against It, Your Children Aren't Old Enough, and There's Nothing You Can Do Anyhow. This different kind of guide by Catalyst, a clearinghouse for women in New York City, gives special insights into concerns of the mature or potential re-entry woman along with a lot of useful career information for women seeking part-time jobs. For counselors working with women in the second half of their lives. Fireside Books, Simon and Schuster, 1972.

NEA Resource Kit on Sex Roles in Education is a series of materials being produced during 1974 by the Resource Center on Sex Roles in Education, National Foundation for the Improvement of Education, Washington, D.C. 20036. It consists of multimedia materials under development (including a film called Cinderella Is Dead) and a newsletter to assist in reduction of sex-role stereotyping.

Other Printed Resources

"Guidelines for Assessment of Sex Bias and Sex Fairness in Career Interest Inventories," July 1974. National Institute of Education, HEW, Washington, D.C. 20208. Guidelines were the outcome of a conference on the same topic sponsored by NIE and involving test constructors, publishers, and users.

U.S. Department of Labor has published several brochures and booklets on the role and status of women. Among the most pertinent are Women in the '70s, Twenty Facts on Women Workers, Careers for Women in the '70s, The Myth and the Reality, Steps to Opening the Skilled Trades to Women, and Women Workers Today, 1973.

Several special journal issues in counseling and related fields have been devoted to topics related to women's development and counseling women. Among them are "Counselors and Women," APGA, December 1972; "Counseling Women," APA, Volume 4, No. 1, 1973; "Changing Women in a Changing Society," American Journal of Sociology, University of Chicago, 1972; "Counseling Girls and Women over the Life Span," NVGA, 1972; Women and

ACES: Perspective and Issues, ACES, 1974, Commission for Women, New Research on Woman, University of Michigan, Ann Arbor, 1974.

Among books especially useful are Saturday's Child, statements of 36 women who work and like it, a kind of psycho-social picture by photographer Suzanne Seed, Bantam, 1973; Julia Sherman's On the Psychology of Women, Charles C. Thomas, Springfield, Illinois, 1971; Juanita Kreps' Sex in the Marketplace: American Women at Work; and Cook's and Stone's Counseling Women, Houghton Mifflin, 1973.

Questionnaires

Counselors and teachers often are searching for instruments to look at their own attitudes, to use in workshops on sexism, and for use with students. Among the shorter ones intended to stimulate discussion and create consciousness are the Birk and Tanney Opinionnaire on "Myths and Realities," 1973; the DeKock questionnaire on male-female attitudes, Interact, Lakeside, California; and the Pat Engelhard questionnaire which served as a basis for a statewide study of counselor attitudes (Impact, 1972), the latter self-scoring. The major instrument on which considerable research exists and which has been validated is the Spence and Helmreich Scale of Attitudes toward Women's Roles, Psychological Abstracts, 1973.

Games, Exercises, and Simulations

Herstory. A simulation by Paul DeKock of male and female roles emphasizing the American woman's circumstances, past and present. Students follow four cycles of learning including surveys, contact projects, research, role playing, and discussion. The author classifies 44 hypotheses about male and female roles into four study cycles--Manners and Courtship; Marriage, Family, Divorce; Job, Achievements, Reform; Nature or Nurture. Interact, Box 262, Lakeside, California 92040.

Joan and Paul. A teaching guide for role-playing approach to career counseling. Attempts to show through decisions students make for Joan and Paul what might happen to a woman who decides to combine marriage and an academic career. The two make decisions individually and together for further education, work, marriage, and children at different decision points. Can be adapted for counseling or teaching. Emma Willard Task Force on Education, 1520 West 27th Street, Minneapolis, Minnesota 55408.

When I Grow Up I Want To Be Married. A series of 10 female profiles based on actual facts about women's life expectancy, educational-vocational-family decisions, and participation in the work force. Can be used for inservice workshops with teachers and counselors or with students themselves. Participants build an "ideal life" for their profile person. Impact, Counseling and Personnel Services Center, University of Michigan, Ann Arbor, Michigan, 1972.

Career Exploration for High School Women. A Model. Authors Janice Birk and Mary Faith Tanney have created a three-day program for high school women to explore new career and lifestyle possibilities. The optional

program includes several exercises, including a Living Careergram, Myths and Realities, Group Fantasy of "What Can Be," and Roleplaying, 1973. Dr. Janice Birk and Dr. Mary Faith Tanney, Counselor Education Department, University of Maryland, College Park.

The Counselor and the Feminine Protest. A booklet with background articles on male and female roles, with perspectives on the problems from both sexes. Main help for counselors is a role-play situation in which two exaggerated counseling interviews are presented to help counselors identify biases, attitudes, and behaviors which are not supportive of women, some subtle, some obvious. "The Biased Counselor" and "Pure Fantasy." Counseling and Personnel Services Center, University of Michigan, Ann Arbor, December 1971, CG 400 034.

The Role of Women in American Society. A film-based curriculum for male and female students aimed primarily at 10th, 11th, and 12th grades. Purpose is "to help adolescents examine the decision points in women's lives so that they may make informed decisions about their own lives." It consists of ten units plus teacher and student resource guides. It could serve as a base for a one-semester course or for mini-courses. Materials are under development from 1973-1975 and are being field tested. Ms. Adeline Nairman, Education Development Center, 55 Chapel Street, Newton, Massachusetts 02160.

Learning Kit for Guidance Counselors and Counselor Educators to Aid in the Deliverance of Sex-Fair Counseling. This is a self-administered six-hour training kit designed to help secondary and post-secondary counselors avoid sex stereotyping in their guidance activities, particularly in career counseling. Intended to be used individually or in groups, it will consist of five modules on orientation to the need, the laws and regulations, bias in inventories, bias in interview content, and resource guide. Carol B. Crump, Abt Associates, Inc., 55 Wheeler Street, Cambridge, Massachusetts 02138.

APGA Project on Sex Equality in Guidance Opportunities. A nationally funded project to help involve counselors around the nation in eliminating sexism and creating greater sex equality for women. It consists of a training conference for one trainer selected in each state to go back and offer workshops for counselors. Plans also call for development of a counselor kit of media and resources in 1974-75. Mary Ellen Hilliard, Project Director, 1607 New Hampshire Avenue N.W., Washington, D.C. 20009.

Films, Filmstrips, Video Cassettes

On Women. A series of audio cassettes on various aspects of women's issues, including assertiveness training, consciousness raising, support groups, and the like. Joan Hamacheck. Michigan State University, East Lansing, 1973.

Career Development of Women. A series of half-hour color video cassettes to assist in inservice workshops, courses on women, and counselor education. The first four deal with (1) Life Career Patterns of Women, (2)

Male Perspectives on Women's Changing Roles, (3) New Directions: Junior High, and (4) New Directions: Senior High. Developed by L. Sunny Hansen, Counseling and Student Personnel Psychology, 139b Burton Hall, University of Minnesota, Minneapolis 55455, 1974.

Martha Stuart's Are You Listening? A TV social action series with several 20-25 minute color video cassettes and film available on topics related to women (welfare mothers, women who have had an abortion, college women, women in management, etc.) Martha Stuart Productions, New York, New York. For rent or sale.

Anything You Want To Be. A 10-minute stimulus film in black and white, this can be used with counselors or with students directly. It shows the ironies of a high school girl growing up in America. Diane Brandon, New Day Films, 267 West 5th Street, New York, New York.

Modern Women: The Uneasy Life. A one-hour documentary, this film explores the feelings of college-educated women about the various roles available to them today. Interviews are conducted with traditional mothers and homemakers, those combining roles of work and marriage, and professional single women. The women discuss the satisfactions and frustrations of their lives and patterns. Attitudes of husbands and single males toward educated women are also explored.

Free to Be You and Me. Both a 33 rpm record and a paperback book, this is an excellent audio-visual resource, especially for the elementary level but enjoyed by adults as well. Based on the TV program of the same title by Marlo Thomas, it attempts to break down stereotypes through poems, songs, stories, and a new kind of fairy tale. Sample titles: William's Doll, It's All Right to Cry, My Dog Is a Plumber, Brothers and Sisters. New York: McGraw Hill, 1974.

Assertiveness Training. Pat Jakobowski Spector. Two 20-minute training films produced to assist in women's courses and counseling women to be more assertive. A series of short open-ended situations are presented, with opportunity for the viewer to practice his or her own responses. APGA, 1607 New Hampshire Avenue N.W., Washington, D.C.

Racial and Sex-Role Stereotyping in Career Information Illustration. This slide presentation gives the results of a study carried out at the University of Maryland on racial and sex stereotyping in selected occupational literature, particularly the Occupational Outlook Handbook, the Encyclopedia of Careers and Vocational Guidance, 1972, and the Science Research Associates Occupational Briefs, 1973. The investigators conclude that women and minorities are underrepresented and that the realities of the work world are not accurately presented. Women and minorities appear in subordinate, assistant, and passive roles. Janice M. Birk, Jacqueline Cooper, and Mary Faith Tanney, University of Maryland, College Park, 1973.

Dick and Jane as Victims and Women on Words and Images. A 25-minute slide-tape presentation, this presents the results of a study of textbooks used in New Jersey public schools by the New Jersey Commission on the Status of Women. The slides vividly portray the programming which both boys and girls experience in elementary and junior high textbook images. No acceptable sets of texts were found among 14 sets examined. Accompanied by a printed booklet. New Jersey Commission on Women, 1972.

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Appendix D: Teaching Resources

While we expect instructors will bring their unique perspectives to the course they teach, we have included our course descriptions and a list of audio-visual resources to stimulate you to develop a course on Women in Science and to facilitate this development. In addition to these resources, we recommend assembling a list of local women scientists who can contribute significantly to classroom discussions about the nature of science and the lifestyles of scientists. We also believe that scientific investigative skills should be applied to teaching programs as well as to research problems. We have therefore included a bibliography on evaluation procedures to encourage you to look carefully at the effectiveness of your program.

COURSE DESCRIPTION OF SCIENCE, SEX, AND SOCIETY

SCIENCE, SEX, AND SOCIETY, an academic course for first-year and second-year students, is designed to encourage students to explore opportunities in science when choosing individual lifestyles that will be most satisfying to them. This course is intended to counterbalance social pressures that in the past have discouraged women from entering careers thought to be incompatible with "femininity."

The course includes:

- ° Practice in the methods of scientific investigation
- ° A survey of the history of women in science
- ° Conversations with contemporary women scientists
- ° Discussions of sex bias in scientific research, training, and career patterns
- ° Exploration of routes and resources for becoming a scientist
- ° Laboratory experience with scientific equipment
- ° Assessment of personal skills and career values

The class will discuss selected readings on the following topics:

- I. Becoming a scientist: the education of women scientists
- II. The nature of science
 - a. The search for the structure of DNA
 - b. Rewards of being a scientist
 - c. The nature of women as studied by scientists
- III. Lifestyles of scientists
 - a. Family and work
 - b. Career choice and options
- IV. Medicine, an example of a science-based profession that women can practice, that studies women, and that provides services to women

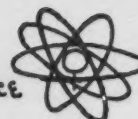
The laboratory work, which is based in part on the contributions of women scientists, provides opportunities to use modern equipment and to learn about scientific techniques.

Prerequisites: None. No college-level science background will be assumed.



SCIENCE, SEX, & SOCIETY

A COURSE ABOUT WOMEN IN SCIENCE



FRESHMEN & SOPHOMORES WHO HAVE TALENTS IN SCIENCE, BUT WONDER IF IT IS THE RIGHT CAREER FOR THEM

OR

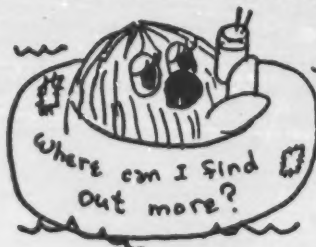
ANYONE WHO'S INTERESTED IN EXPLORING THE INTERACTIONS AMONG SCIENCE, SOCIETY, AND THE LIVES OF INDIVIDUALS



THE COURSE IS OPEN TO BOTH MEN AND WOMEN SPRING SEMESTER - IT WILL BE LISTED AS:

BIOLOGY 235
TOPICS IN BIOLOGY:
SCIENCE, SEX & SOCIETY
3 CREDIT HOURS
SPRING 1978

- A CHANCE TO LEARN ABOUT WOMEN SCIENTISTS TODAY & YESTERDAY
- EXPLORATION OF CAREER OPPORTUNITIES IN ALL AREAS OF SCIENCE
- "HANDS-ON" LABORATORY EXPERIENCE & A CHANCE TO LEARN ABOUT METHODS OF SCIENTIFIC INVESTIGATION
- DISCUSSION WITH WOMEN SCIENTISTS ABOUT THEIR CAREERS, EDUCATION, AND FAMILIES



IF YOU'RE INTERESTED, SAVE THIS ANNOUNCEMENT UNTIL YOU SEE YOUR ADVISOR NEXT FALL - FOR FURTHER INFORMATION, CONTACT:

DR. ANN E. KAMMER
DIVISION OF BIOLOGY
532-6646

OR CHERYL N. GRANROSE
DIVISION OF BIOLOGY
532-6615

STUDENT SKILLS: BEHAVIORAL OBJECTIVES FOR COURSE ON
SCIENCE, SEX, AND SOCIETY

<u>Student Skills</u>	<u>Course Experience for Learning these Skills</u>
I. Understand the work of professional scientists.	LAB, Section III
A. Use scientific method. 1. Formulate hypotheses. 2. Identify scientific method in work of others. 3. Distinguish valid from invalid evidence. 4. Identify logical conclusions. 5. Apply scientific method or logical thinking to problems encountered.	Section II & III
B. Identify the ongoing nature of the scientific process. 1. Distinguish contributions of various people to one scientific advance. 2. Correlate the finding of several scientists.	Section II & III
C. Demonstrate familiarity with scientific instruments. 1. Successfully operate at least 2 pieces of laboratory equipment. 2. Identify situations in which these instruments would be useful. 3. List types of information produced by each instrument. 4. Express willingness and interest in operating instruments.	LAB
D. Appreciate the role of serendipity in science. 1. Identify an example of the benefits of chance observation. 2. Demonstrate willingness to receive unexpected information.	Section II, III
E. Recognize sex bias in scientific investigation. 1. Distinguish between biased and unbiased research. 2. Identify social consequences of sex bias for recipients of scientific services.	Section III
F. Appreciate the lifestyle of a professional scientist. 1. Describe the work week of a scientist.	Section III, IV Biographies, Interviews

<u>Student Skills</u>	<u>Course Experience for Learning these Skills</u>
2. List several factors often considered necessary for advancement in a scientific career.	
II. Know contribution of women to scientific progress.	Section II, III
A. Identify major contributions of selected women scientists.	Biographies
B. Recognize historical trends in scientific careers for women.	
C. Identify characteristics of an individual woman scientist one might emulate.	
III. Identify factors that influence scientific career choice.	
A. Appreciate professional prerequisites for pursuing a successful scientific career.	Section IV
1. Identify necessary requirements for becoming a scientist.	
2. Identify routes for entering a scientific career.	
3. Identify resources available to assist in pursuing a scientific career.	
B. List personal factors influencing career choice.	
C. Identify the role of social stereotyping in career choices.	
D. Distinguish between external and internal determinates of career choice.	
E. Clarify personal values influential in career decisions.	
1. Accept nontraditional careers as options for self or others.	
2. Evaluate consequences of personal scientific career choice.	
3. State nonsexist views when discrimination is encountered.	
4. Apply decision-making skills to career choice.	
5. State and weigh values on which personal career choice will be made.	

LABORATORY EXERCISE 1: INTRODUCTION TO THE SCIENTIFIC METHOD

The objective of this exercise is to gain a better understanding of the nature of science. Science is a process, a way of finding out about the world, as well as the information accumulated as a result of that process. We shall use the process of putting together a jigsaw puzzle as an analogy of science.

Connection to Reality: Rosalyn Yalow

Scientific puzzles and the procedures used to solve them take many forms, as illustrated by the history of science and the biographies of scientists. Sometimes pieces of the puzzle are difficult to obtain, and the crux of a scientific advance may be a new method for obtaining information.

The puzzle that challenged Dr. Rosalyn Yalow and Dr. Solomon Berson, her collaborator at the Veterans Administration Hospital in New York City, was the cause of a type of diabetes that develops in adults. They wanted to study the amount and fate of insulin in the blood of diabetic and nondiabetic people. (Insulin is a hormone that controls the level of blood sugar and influences cellular metabolism of sugar.) The pieces of the puzzle that they obtained with their initial experiments suggested that patients treated with insulin had developed an antibody that bound the insulin, just as the body normally develops antibodies in an immune or allergenic reaction. Because the suspected insulin-antibody would be present in very small amounts, they developed an extremely sensitive technique to demonstrate that such an antibody was in fact present. Because the antibody binds to insulin, it makes the hormone ineffective, and patients with this type of diabetes cannot easily be treated by injections of insulin. At first other scientists working on this and related puzzles did not accept the results and ignored the method. Subsequently, however, the technique that Yalow and Berson devised, called radioimmunoassay, proved to be widely applicable to studies of other hormones, which are difficult to measure chemically because they are present in very small amounts. The technique has improved the diagnosis of pathological states in which the amount of hormone in the blood is abnormal. It has made possible studies on the regulation of hormone secretion and led to the discovery of new hormones. It is used in detecting blood infected with hepatitis virus and may have many applications in basic research and in public health work. For her role in the development of the radioimmunoassay technique, Rosalyn Yalow was designated a Nobel laureate in Physiology--Medicine

in 1977. While she was in Stockholm, Sweden, to receive the prize, she addressed a Nobel banquet audience.

Many newspapers carried excerpts of her remarks, including the following: "We cannot expect in the immediate future that all women who seek it will receive full equality of opportunity. But if women are to start moving toward that goal, we must believe in ourselves or no one else will believe in us, we must match our aspirations with the competence, courage and determination to succeed, and we must feel a personal responsibility to ease the path for those who come afterwards."

The puzzle that Rosalyn Yalow initially investigated with radioimmunoassay techniques comprised only a small section of two larger puzzles. One is understanding the coordination of the machinery of the body, and another is the maintenance of health. Rosalyn Yalow and many others continue to work on these puzzles. The new technique of radioimmunoassay is fostering rapid progress in many areas, and difficult pieces are falling into place.

Method

Obtain a jigsaw puzzle in a plain box, i.e., with no information about the picture to be assembled. The box may or may not contain all the pieces to a puzzle, and it may or may not contain pieces from more than one puzzle.

Work on the puzzle in groups of three to four (depending on class size and number of puzzles). Begin by selecting one person to be group recorder and to time the progress of the group, recording when you begin, when a piece is added to another, and when you complete a major task. Observe your method of proceeding. Note both your thoughts and the physical manipulations you perform. Make a written list of what you do to solve the puzzle.

After a half hour stop working on the puzzle and compare your list with those of your colleagues. Then return to the puzzle, noting if new relationships are apparent when you first start to work. If you learn new strategies from your colleagues, try to apply them. The group should continue to record the times at which pieces are correctly placed in the picture.

After another 20 minutes, make a graph of your progress. Label the horizontal axis "time in minutes" and let time 0 be the moment you began work. Label the vertical axis "assembly rate," determine the number of pieces added to the picture each minute (or each five minutes if progress is slow), and plot these on the graph. Compare the graphs obtained by different groups of students.

Discussion

The following questions are intended to stimulate your thinking about what you have done while you worked on the puzzle and to help you compare puzzle solving with science. If you do not yet have enough information about scientific work to answer these questions, keep them in mind as you learn more about scientific methods and their application by individual scientists, and return to them at the end of the term.

1. Did you develop new strategies on your own as you worked on the puzzle?
2. Were you able to learn and apply new or different strategies from your colleagues? What do you think are the roles of personal interactions (cooperation, competition, sharing of techniques and ideas) in the progress of science?
3. Explain the graph. Do you expect similar differences in the rate of scientific progress with time? Why or why not?
4. Make any comparisons you can among (1) your solving of the jigsaw puzzle, (2) Rosalyn Yalow's scientific work, and (3) other scientific work known to you. What do you think are the relative roles of development of a technique versus insight into a solution? How is the rate of progress affected by a new approach? Is there a high probability that mistakes will be discovered?
5. What are some similarities between the scientific method and what you have done in working with the puzzle? What are some differences between solving jigsaw puzzles and solving scientific problems?

References

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Other Games

1. "Mastermind," described as a battle of wit and logic between two players. Available commercially from Invicta Plastics (U.S.A.) Ltd., Suite 923, 200 Fifth Avenue, New York, N.Y. 10010. A similar game can be played with colored pens and paper; substitute "Hypothesis" for the Codebreaker's moves and "Data" for the response given by the Codemaker. The essence of the game is devising a good strategy to establish the arrangement of four colored pegs, given indirect evidence.
2. "The New Eleusis," devised by Robert Abbott, P.O. Box 1175, General Post Office, New York, N.Y. 10001. Rules are available from him for \$1.00 for the first copy and 25¢ for each additional copy. The game was discussed by Martin Gardner in the October 1977 Scientific American. Its essence is conceptualizing the rule governing the sequence of playing cards laid down by the codemaker.

LABORATORY EXERCISE 2: SURFACE TENSION

Have you seen water striders moving about on the still surface of a pond? Have you tried the "magic" trick of floating a sewing needle on the surface of a glass of water? If you have, you know that neither insect nor needle is floating like a piece of wood; both sink if they become wet and break through the water surface. The water surface is peculiar, behaving like an elastic film. Water and other liquids have this property, called surface tension, because molecules at the surface are subject to attractive forces from the sides and below but not from above where there are no like molecules. In contrast, molecules in the middle of the solution are subject to attractive forces from all sides, since there are like molecules all around them. The magnitude of the surface tension depends on the forces between the molecules of the liquid. It can be altered by bringing the surface in contact with other materials. Surface tension and other aspects of surface chemistry are important in science, technology, and everyday life. Examples are studies on cell membranes, preparation of emulsions, waterproofing of textiles, oil films on water to reduce evaporation of reservoirs in arid regions, and application of agricultural sprays.

The purpose of this exercise is to devise some experiments on surface tension, using common materials.

Connection to Reality: Agnes Pockels

One of the early contributors to our understanding of surface tension was Agnes Pockels, whose initial work was done in her kitchen, using homemade apparatus. She grew up in Germany during the last half of the nineteenth century, before women were allowed to study at a university. Her first duty was to care for her family, and her early work was stimulated by observations on greasy dishwater. She performed careful experiments on the effects of oil on the surface tension of water. She used a fine powder to detect a surface film that is otherwise invisible. She devised a way to measure surface tension: a balance composed of a thin, light bar of wood pivoted on a straight pin, with one arm supporting a porcelain button or metal ring attached to silk thread by a thin wire, and the other arm counterbalanced by a movable strip of metal. The balance was arranged so that the button floated on the water surface.

By shifting the counterbalancing metal strip, Pockels measured the force needed to lift the button from the surface. She also devised a way to vary the thickness of an oil film on the water. A rectangular tray was filled with water and a thin strip of tin was laid across it. By shifting the tin strip, the surface area was changed, and the oil either spread out or piled up on the water surface. Using the balance Pockels measured the effect of these changes on the surface tension.

Through the support of her brother Frederick, who received a doctorate in physics and an appointment of Göttingen University, and through the support of a well-known English scientist, Lord Rayleigh, who had similar interests, Pockels published her first paper in an important English journal, Nature, in 1841. Subsequently she published additional papers in Nature and in German journals. She was offered research facilities at Göttingen University, and in 1932 she was awarded an honorary doctorate degree by the university in her native town of Braunschweig.

In the same year, 1932, an American chemist, Irving Langmuir, received the Nobel prize for his work on surface chemistry. Much of his research was an extension of Pockels' pioneering experiments on oil films and water.

Methods

Observations on surface tension and the interaction of oil, water, and detergents can be made using simple techniques similar to those employed by Agnes Pockels. Exercise your ingenuity in devising observations to make, hypotheses to test, and equipment to use.

A balance for obtaining relative measurements of surface tension is described above and in Pockels' papers. Different data can be obtained by observing the spread of talc papers, or the movement of a waxed thread on a water surface. Can you observe the effects of a surface film that is not directly visible? Experiment with different household oils and detergents (bring your favorite brands). What kinds of liquids make good surface films on water?

Some procedural hints may save you time. Use small quantities of materials to form or indicate surface films. If comparisons are to be made, use the same quantity of each material, e.g. drops of oil. Use a clean container; a residue of soap or dust can change the surface properties of the water in the container. Repeat experiments as a check on your observations.

After some exploratory experiments, think about ways of measuring rather than just watching, and think about the advantages and disadvantages of measuring. If time permits, construct a balance similar to that used by Agnes Pockels, or devise your own method for measuring surface tension.

As you proceed, make notes about your observations. At the end of the exercise, summarize your results in a brief paragraph of objective statements. Distinguish carefully between fact and interpretation. Select one observation, decide if it is a fact, and offer two different interpretations of the observation.

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LABORATORY EXERCISE 3: AN INTRODUCTION TO DATA-GATHERING AND STATISTICS

Scientific research often involves the collection and analysis of data. The study of a particular phenomenon begins by gathering and analyzing information so that a hypothesis can be formulated. This hypothesis is tested by collecting data and then describing it with appropriate quantitative measures.

The purpose of this exercise is to provide you with an opportunity to gather some data and describe it with the "chi-squared test."

Connection to Reality: Bonnie Benincasa

Do non-smokers live longer than smokers? How does stress affect mortality or morbidity? Insurance companies are constantly gathering and analyzing data in order to measure the influence of such factors such as these on your health. There is a specialized staff of professionals, called actuaries, who work in the insurance field to develop mortality tables and other statistical models tailored to the needs of the insurance industry. Bonnie Benincasa is an actuary working for John Hancock Mutual Life Insurance Company in Boston. She is a "Fellow" in the Society of Actuaries, a designation that is earned by passing a series of nine examinations. The examinations represent a standard of professional expertise that is recognized internationally. The "tools of the trade" acquired from the examinations include not only mathematical subjects such as numerical analysis and risk theory, but also other technical subjects such as insurance law, taxation, demography, accounting, and valuation of liabilities.

Bonnie is an assistant actuary on the Individual Equity and Pension Products Staff. Her job is constantly changing--from securities accounting and financial reporting to individual taxation, reserve valuation, and SEC and government compliance. Presently she is designing a new pension administration system. In addition to membership in The Society of Actuaries, The American Academy of Actuaries, and Boston Actuaries' Club, she serves on the Life Office Management Association's Equity Products Administration Committee and she is a National Association of Securities Dealer (NASD) Principal--an SEC requirement for approving variable annuity applications.

Method

Assume that you want to know if the enrollment of women in a given science course has changed significantly during the last five or ten years. Select a science course that has a reputation for being a difficult introductory course in a

subject traditionally considered masculine. Find out what the enrollment of women and men has been over as long a time-span as possible, in that course and in the college as a whole. The Registrar or the Office of Admissions and Records may have this information, or you may have to consult records maintained by instructors, department heads, deans, or the Affirmative Action Officer.

After the data have been obtained, they must be summarized and analyzed. Graphical presentation will be useful. Let the horizontal axis represent time and the vertical axis some measure of the participation of women in the science course. The best measure is probably the percentage of the class that is women

$$\frac{(\text{number of women in the class} \times 100)}{\text{total number in the class}}$$

because the number in the class varies from year to year. After the graph has been drawn, summarize your results in words.

Further analysis depends on the questions asked and on the choice of an appropriate statistical measure. Suppose you wish to know if the enrollment of women in the science course differed from that expected on the basis of total enrollment of women in your college. Assume as the hypothesis to be tested that men and women are equally likely to enroll in the course. Then if the student population is 50% women, you would expect that 50% of the class will be women. However, random fluctuations can influence the number of students in any particular class.

For example, if you flipped a coin 50 times, you probably would not get 25 heads and 25 tails, although you would expect heads and tails to occur equally often. A simple statistical test that allows you to assess the probability of random fluctuations causing an error because the sample is small is the "chi-squared test." Calculate χ^2 as follows for this example.

Let f_1 = number of women in the class

f_2 = number of men in the class

F_1 = number of women expected

F_2 = number of men expected

Then

$$\chi^2 = \frac{(f_1 - F_1)^2}{F_1} + \frac{(f_2 - F_2)^2}{F_2}$$

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Since $(f-F)$ is a measure of the deviation of the observed class from the expected, then the larger the value of χ^2 , the greater is the probability that the observed deviation is not a random event. The χ^2 value that gives a 95% chance of the deviation being real and not a sampling error or random fluctuation is a value equal to or greater than 3.84 for this example, when we are comparing only two categories--male and female. This test value can be found published in χ^2 tables according to the amount of error one is willing to accept and the number of categories one is testing. If you wish to explore this further, a χ^2 table would be necessary to help you learn how to look up test values for other cases. In this table, the number of categories minus one equals the degrees of freedom (df).

$$\text{number of categories} - 1 = df$$

Once the values of all categories except one is known, the last one must also be known, that is, it is not "free" to vary.

In our example, $df = (2-1) = 1$. The test value (3.84) will be found in the table under 1 df and .05 probability of error since we want to be certain that these results would not occur by chance alone more than 5% of the time.

To make the calculation it is necessary to have some values for F, the expected number. If you assume that women and men are equally likely to enroll in the course, then the expected number depends on the college enrollment. If the student body is 40% women, then according to the assumption the class would be expected to have 40% women. That is

$$F = \frac{\begin{array}{c} \text{(number of women students} \\ \text{in the college)} \end{array} \begin{array}{c} \text{(number of students} \\ \text{in the class)} \end{array}}{\text{total number of students in the college}}$$

For example, consider the following data for college Xxxx:

Total number of men	1740
Total number of women	<u>1260</u>
Total enrollment in 1977	3000
Number of men in beginning calculus, Fall 1977	242
Number of women in the same class	<u>58</u>
Total in the class	300

In this example, the student body is $\frac{1260}{3000} = 42\%$ female.

Therefore the calculus class would be expected to have $(.42)(300) = 126$ women enrolled and $300 - 126 = 174$ men. These numbers are the expected values of $(F_1$ and $F_2)$. Then

$$\begin{aligned}\chi^2 &= \frac{(58-126)^2}{126} + \frac{(242-174)^2}{174} \\ &= 36.69 + 26.57 \\ &= 63.26\end{aligned}$$

Since this value (63.26) is greater than the test value of 3.84, you can conclude that it is highly probable that in Xxxx college in 1977 women are not as likely as men to enroll in the beginning calculus course as would be expected from their percentage of the total school population.

Suppose that you wish to know if more women are enrolling in calculus now than several years ago, for example in 1977 compared with 1972. Obtain enrollment figures and calculate χ^2 for those data. To do this you will want to set up a contingency table in which the rows will be the number of men and women and the columns will be the years. For example:

	1972	1977	Row Totals
Men	158	242	400
Women	42	58	100
Column Totals	200	300	500 Grand Total

As before, $\chi^2 =$ the sum of $\frac{(f-F)^2}{F}$ calculated for all values

of the observed numbers (f) and the corresponding expected numbers (F). A contingency table allows you to determine whether or not any variation in the rows is independent of any variation in the columns. In this example you can test the hypothesis that the proportion of women in the two years is the same (that is, the variation in years does not influence variation in the number of women and men). The best estimate of the expected proportion is the proportion in the combined sample for both years. To find any F , you can multiply the row total, for the row in which the F is found, times the column total for the column in which the F is found, and divide by the grand total.

$$\text{The expected enrollment of men in 1972} = \frac{(400)(200)}{500} = 160$$

$$\text{The expected enrollment for men in 1977} = \frac{(400)(300)}{500} = 240$$

$$\text{The expected value for women in 1972} = \frac{(100)(200)}{500} = 40$$

$$\text{The expected value for women in 1977} = \frac{(100)(300)}{500} = 60$$

If you subtract each expected value from each observed value you will have a table that looks like this:

f	F	f-F	(f-F) ²	$\frac{(f-F)^2}{F}$
158	160	-2	4	.025
242	240	2	4	.017
42	40	2	4	.100
58	60	-2	4	.067

$$\chi^2 = 2.09$$

Because of the way the expected enrollments were calculated, all four deviations (f-F) are equal (except for the sign).

Compare this χ^2 value to the χ^2 table value to see if it is larger than the value expected by chance. In the case of a contingency table, the degrees of freedom equal (number of rows - 1)(number of columns - 1). The df = (1)(1) = 1, the same as the first example. Therefore the same test value can be used.

Since the χ^2 value is smaller than the test value of 3.84, we conclude that the percentage enrollment of women in the beginning calculus course did not change significantly between 1972 and 1977.

There are other simple comparisons that can be made with the χ^2 test, and you should choose ones appropriate to your institution and your interests. Rather than data on students, for example, you could gather data on women scientists versus men scientists in ads for scientific equipment or in popular news stories. In doing so, be aware that statistical tests are devised on the basis of certain assumptions that have not been discussed here, and proceed with caution. Also be cautious about the rights to privacy and informed consent if you want to gather data by asking questions of individuals.

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Women Mathematicians

(Because statistics is a branch of applied mathematics, some references on women mathematicians are included to provide the "connection to reality.")

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LABORATORY EXERCISE 4: COMPUTERS

Computers--sophisticated electronic counting machines--allow scientists, businesspeople, government agents, and even individuals to store and process vast quantities of data and to play old games in new ways. Like any other product of science and technology, computers have potential for good and for ill. A current example of the latter is the prospect that the government can compile various computerized data banks, including income tax information, and thereby assemble detailed dossiers on citizens without their knowledge and consent, a fearsome invasion of privacy. On the other hand, computers perform many useful deeds, such as the rapid processing of airline reservations or other business information, and computers have made possible scientific experiments that would otherwise be impossible because of the large quantities of data required.

Connection to Reality: Ada Bryon Lovelace

The development of computers, including their increased speed and complexity, has paralleled the development of electronic devices. The original computer, however, was a mechanical device, the Difference Engine, and later, the Analytical Engine. Both were developed in London during the first half of the nineteenth century by an English scientist and inventor, Charles Babbage. Among the visitors to Babbage's studio in 1833 were Mary Somerville, who was already famous for her popular book on higher mathematics and astronomy, Mechanisms of the Heavens. With Somerville was her young friend, Ada Bryon. Subsequently Ada Bryon Lovelace, who had developed her mathematical talents in spite of the lack of societal support for such activities by women, became Babbage's collaborator. She wrote the best contemporary account of the theory and operation of Babbage's machine. She was the first person to describe in detail the process by which a computer is programmed. Information was supplied to the Analytical Engine in the form of holes punched in cards, just as the Jacquard loom was programmed to weave patterned fabrics. Lovelace was optimistic about the capabilities of computing machines, but she was also aware of their limitations. Computers can process data and obey instructions, but they can not originate anything not already programmed. Babbage and Lovelace later collaborated in devising a scheme to win money betting on horses, but they were not successful, and Lovelace lost large sums and went badly into debt.

Objectives and Procedures

Because computers are an important component of many scientific projects, business operations, and industrial control systems, some familiarity with their capabilities is desirable. Arrange a visit to a large computing facility and also to a laboratory employing a small computer or microprocessor. Inquire about the use of the machine and the principles of operation.

If possible, run a simple program on a computer. You will have to find out how to talk to the machine; each system has its language and idiosyncracies. First find out what code words indicate to your computer that a program is coming. If your program is to be presented to the computer on punched cards, obtain some cards and learn to use a keypunch machine. This machine has a keyboard like a typewriter, and it writes one line of type at the top of the card while punching the corresponding set of holes. Because the arrangement of the holes on the card signals the computer, it is essential that the format be followed exactly, including the column used to start, the spacing, and the use of commas. You will need to provide an initial communication telling the computer that your program is coming, the program itself, and the data on which the program is to operate.

Given below is a simple program for computing an average of three numbers. The program is written in the WATFIV language, an easy-to-use version of FORTRAN. The program causes the computer to:

1. Read three numbers into variables called A, B, and C.
2. Print the message "THE THREE NUMBERS ARE," followed by the values which were read for A, B, and C respectively. (This is an echo check.)
3. Calculate: $AVG = (A+B+C)/3$.
4. Print "THE AVERAGE IS" followed by the value of the variable AVG.
5. Print "NORMAL TERMINATION."

Instead of this program, you may write your own. For example, write a program to do the χ^2 analysis discussed in the previous exercise.

Each line of the program corresponds to one computer card. The cards are punched with the first letter of the line in column 7

```
PRINT, 'PROGRAM 1'  
  
READ, A,B,C  
  
PRINT, 'THE THREE NUMBERS ARE,'A,B,C  
  
AVG=(A+B+C)/3  
  
PRINT, 'THE AVERAGE IS,' AVG  
  
PRINT, 'NORMAL TERMINATION'  
  
STOP  
  
END
```

Note that the program concludes with a command to stop execution of the program and a word that signals the end of the program. Note also that once the program has been written and punched on cards, it can be reused with as many sets of data as necessary. With additional commands it can be stored in the computer for future use.

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LABORATORY EXERCISE 5: ELECTRICITY AND ELECTRONICS

Electricity and electronic devices are familiar but mysterious servants. Although that mystery cannot be dispelled in one or two afternoons, you can learn to solder and to construct a simple electronic device. The latter is most easily accomplished by purchasing a kit from Radio Shack, Heathkit, or a similar supplier. In some kits connections are made by soldering; gadgets ranging from simple radios to color television sets can be made in this way. Other kits are designed to provide practice in building circuits, and the connections are made with spring clips that permit rapid assembly and disassembly. These solderless kits provide components and directions for several different projects and are probably the best way to begin.

To help you understand circuit construction, the following paragraphs review some basic concepts, components, and techniques.

Connection to Reality: Mary Ellen Lewandowski

In 1979, Mary Ellen Lewandowski was at the start of a promising career designing microprocessors and microprocessor-based systems. Microprocessors touch many facets of our lives; they are now found in cars, microwave ovens, toys, sewing machines, televisions, and many other common items.

In high school, Lewandowski was very interested in mathematics and felt she would like to base a career on this interest. During this time she had an opportunity to program a computer, and found that the discipline necessary to do this was similar to the discipline necessary for work in mathematics.

In college, she chose electrical engineering, specializing in computers: the components they are built from, the overall architecture that ties the components together, and the programming that allows people to communicate with computers. Underlying all the electrical engineering studies was a strong dependency on mathematics, her first love.

The technology and pace surrounding the computer industry have allowed Lewandowski to build a dynamic career at Digital Equipment Corporation (DEC). As an engineer, her area of expertise is in designing, building, testing, and documenting microprocessor products. She acquired the basic information necessary to do these tasks in college; her knowledge of microprocessors and their applications has come about through career specialization. In addition, she gained management skills through special courses, college business courses, and on-the-job training. As an engineering supervisor at DEC, she

is responsible for ensuring that all six facets of a product design are in place:

- ° determining the need for a product (Can we sell it?)
- ° defining the details surrounding the product (Should it be green or red?)
- ° calculating the return on the investment (Can the company make a profit?)
- ° designing, building, thoroughly testing and documenting the product
- ° manufacturing the product in large volume for customers
- ° training the sales force and field-service personnel.

Review of Basic Concepts

The flow of electrical current through a circuit is in many respects analogous to the flow of water through pipes. Instead of a pipe there is a wire or other conductor, and instead of flowing water there are moving charges, electrons in a metal or ions in a solution (e.g., a lead-acid battery). The quantity of electrical charge flowing per unit time is the current, measured in amperes. Materials differ in their ability to conduct current, just as metal conducts heat better than does wood. In addition, any material, e.g., a metal wire, will be more resistant to the flow of current when it is longer and thinner. This resistance to current flow is called resistance and is measured in ohms. To move electrical charges through a resistance, energy is required, just as it is required to move water or to do any work. Energy is available when there is a potential difference, measured in volts, between two points in the circuit. For example, a flashlight battery is so constructed that it has a potential difference of 1.5 volts between its two terminals. This voltage can force electrons through the high resistance wire in a flashlight bulb, and because of the work the wire becomes hot and glows. The battery produces a flow of electrons only in one direction, and until it runs down or the bulb burns out, the current is constant. This constant current is called direct current (DC). By convention DC current flows from the positive side to the negative side of the source of potential difference, such as the battery.

Because of their polarity, batteries usually must be connected properly to the other components in the circuit. For example, when you replace the batteries in a portable radio, you must insert the batteries in a given orientation. On the

other hand, when you connect the radio to the line voltage by inserting a two-pronged plug into the socket on the wall, the orientation of the plug does not matter. The power line provides alternating current (AC), an electrical flow that changes direction rapidly. In the U.S., household current changes polarity 60 times per second and the potential difference between the two sides of the socket is 110-120 volts. The quantity of current drawn depends on the resistance of the gadget that is connected; for example a toaster draws more current than does a small light bulb.

Some Components

The following components may be encountered in constructing electronic equipment.

1. Resistor -- a component constructed with a fixed resistance or a resistance that can be varied by turning a knob or moving some sliding part. Fixed resistors are marked with rings of color that indicate their resistance. Initially you may prefer to determine the resistance by measuring it with an ohmmeter, rather than learning the color code.
2. Capacitor -- a device consisting of two conducting layers (or plates) slightly separated by a nonconducting material. When a steady potential difference is connected across a capacitor, one plate becomes positively charged and the other negatively charged. Thus a capacitor can store charge. The amount of charge that it can store per volt is its capacitance, measured in farads. Since capacitors take time to charge and discharge, they can be used in combination with resistors to control the rate of DC current flow. When AC current is applied, however, it changes polarity too quickly to charge the capacitor fully. In effect the capacitor allows AC current to pass. Capacitors can be used to direct the flow of AC current, depending on its frequency (changes in polarity per second) relative to the size of the capacitor.
3. Transistor -- an electronic device made of layers of a special semiconductor material. Transistors are capable of controlling or amplifying the current or voltage between two connections in relation to the voltage applied to a third connection. Transistors open a whole new realm of electronics, one in which shapes, layers, and controlled impurities form structures that store, split, recombine, and otherwise control electric current. Since they can be made very small, and since they operate on little current, their development has led to the proliferation of small, battery-powered gadgets and instruments.
4. Integrated circuit -- an electronic device consisting of miniature resistors, transistors, and sometimes capacitors, interconnected in a circuit on a single wafer of silicon,

usually a few millimeters square, and packed in some protective material. A single wafer or "chip" may support over 100,000 transistors and associated circuitry. Simple calculators and digital alarm clocks may use only a single chip. Sophisticated instruments can have many chips and corresponding complex capabilities, all packed into a relatively small container.

Demonstration of the Behavior of Components

Construct the following circuits using clips (alligator clips or spring clips) or solder to hold wires together. Work on a nonconducting surface.

A. Diode demonstration

Components needed: 6V battery, lamp with leads (lamp should be rated for 6V, Radio Shack 272-1140 or equivalent), 3 wires (stranded No. 20, plastic insulated), 4-6 clips.

A diode is a semiconductor device that allows current to flow readily in one direction but not in the opposite direction. This simple experiment demonstrates the effect of inserting a diode into a circuit.

First test the battery and lamp. Connect a wire from one pole of the battery to one lead of a miniature lamp. Connect a second wire from the other lead of the lamp back to the battery. If the connections are sound, the bulb should light.

Now insert a diode into the circuit. Disconnect one wire from the lamp and connect it to one lead of the diode. With a third wire, connect the other lead of the diode to the lamp. Does the lamp light? Reverse the polarity of the diode in the circuit. Does the lamp light? What would happen if the power were supplied not by a battery but by an AC source?

B. Transistor demonstration

Components needed: 6V battery, lamp with leads (Radio Shack 272-1140 or equivalent), NPN transistor (HEP S 5014 or equivalent), 100 K and 1 K resistors (the exact values do not matter), clips and wires.

Connect the positive terminal of a 6V battery to one lead of the lamp. Identify the leads of a NPN transistor. (The instructions packed with the transistor explain how to tell which lead of the transistor is the collector, the emitter, and the base.) When the transistor is in the circuit, do not let the leads of the transistor touch each other, or too much current will flow and burn out the transistor.) Connect the negative terminal of the battery to the emitter of the

transistor. Does the lamp light? In this configuration there is a high resistance between the collector and emitter of the transistor, and current flow is blocked. The current flow can be regulated by applying a voltage to the base of the transistor. Connect a 100 kilohm resistor between the collector and the base. Does the lamp glow? Remove that resistor and connect a 1 kilohm resistor between the collector and the base. The smaller resistor allows more current to flow, and the lamp should look brighter.

Trace the path by which the current flows from the battery, through the lamp and transistor, and back to the battery. The current flowing through the circuit is controlled by a small voltage applied to the third lead of the transistor (the base, in this case). The smaller resistor between the battery and the base causes a slightly larger voltage to be applied to the base, and more current flows between base and emitter. Although this base-emitter current is very small, it controls the much greater collector-emitter current that powers the light. This relationship is the basic principle of transistor amplification.

Construction of Circuits

If you purchase a kit, read and heed the instructions included. The most common error is a poorly soldered connection.

Hints on soldering

1. Take care of the soldering iron. Its tip should be kept covered with a thin layer of solder. Wipe off any oxidized excess on a damp sponge before you attempt to solder a connection.
2. The wires to be soldered must be clean and shiny. It is advisable to scrape them lightly with a piece of sandpaper or the blade of a knife.
3. All joints should be mechanically firm before the solder is applied. Wrap the wires around each other. Solder is not a glue; its function is to make an electrical connection.
4. Use only 60/40 rosin-core solder. Do not use acid-core solder.
5. Heat the junction to be soldered and then with the soldering iron still in contact with the junction, apply the solder so that it melts quickly and flows onto the wires. Remove the piece of unused solder and the iron. Do not move the wires before the solder hardens (which it should do quickly). The resultant joint should look shiny and smooth, and solder should adhere well to both wires.

6. After the joint has cooled, tug on a wire gently to be sure the connection is firm.
7. When soldering delicate components such as transistors, attach a heat-sink to avoid overheating them. If a special clip is not available, grasp the lead of the transistor with a pair of pliers. With the pliers in contact between the end of the lead to be soldered and the body of the transistor, the heat will flow into the pliers. Do not apply heat for longer than is necessary to cause the solder to flow over the connection.

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LABORATORY EXERCISE 6: HUMAN BLOOD CELLS

Blood cells are probably best known for their role in respiration and in combating infectious disease. The red blood cells, which are the most abundant cells in the blood, transport oxygen from the lungs to all parts of the body. The white blood cells are a heterogeneous group of less abundant cells, some of which become more numerous when the body is fighting an infection.

Connection to Reality: Florence Sabin

Many individuals have contributed to our knowledge of the structure and function of blood cells. One of these people was Florence Sabin, a physician on the faculty of Johns Hopkins University School of Medicine. Dr. Sabin was interested in the formation of red and white blood cells. She devised a technique for keeping the cells alive for several hours outside the body and a vital staining method that distinguished among immature red blood cells and various types of white blood cells. These techniques allowed her to study the development of red blood cells. She also studied changes in the proportion of different cell types in normal blood (her own) and in blood from patients, including a case of leukemia. In addition to her work on blood cells, Sabin studied the embryonic development of lymph vessels, and cellular mechanisms of immune responses, particularly in relation to tuberculosis. She is credited with a long list of firsts, including: one of the first women admitted (in 1896) to the new Medical School of Johns Hopkins University and subsequently the first woman faculty member there (a position she held for 23 years), the first woman President of the American Association of Anatomists, the first woman elected to membership in the National Academy of Science, and the first woman to be a full-time member of the staff of the Rockefeller Institute for Medical Research in New York City, where she worked for 13 years. After her retirement from the Rockefeller Institute at the age of 67, she returned to her native Colorado and was instrumental in establishing a public health program in that state. (A biography of Sabin is included in Section II B.)

Objectives

An objective of this exercise is to stain and observe your blood cells. You will not use Sabin's vital staining technique but a routine procedure in which the cells are killed and preserved.

The staining procedure will introduce you to a method for preparing cells for microscopic examination. You will use a light microscope, which is an important tool in several fields, including biology, medicine, pathology, geology, and metallurgy.

Materials

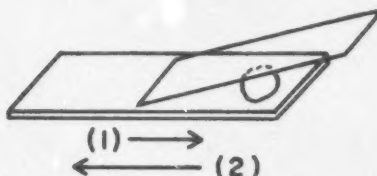
The following supplies are needed: immersion oil, lens tissue, paper towels, sterile lancets, absorbant cotton, 3" by 1" glass microscope slides, 70% alcohol, sets of 5 coplin staining jars filled with: #1 methyl alcohol; #2, eosinate solution; #3, distilled water; #4, polychrome stain; #5, distilled water (#2 and #4 are available as Wrights Dip Stat from Medi-Chem, Inc., P.O. Box 445, Santa Monica, California 90404). In addition each student will need a microscope equipped with a 40x objective. (If the students have had some experience with microscopes, oil immersion lenses can be used to good advantage.)

Method

In this procedure (described in detail below), a drop of blood is obtained from a small finger puncture, spread on a glass slide, and stained with commercially available reagents. Obtain the necessary supplies and understand the procedure before you begin. Work in pairs.

1. Check over your microscope and make sure you understand how to use it. (Your instructor may have supplementary instructions applicable to the type of microscope available.)
2. Obtain several clean, dry microscope slides; handle them by the edges only. Lay them out on a paper towel. Also obtain a sterile lancet, cotton moistened with 70% alcohol, and some dry cotton or absorbent paper.
3. Cleanse the tip of the fourth or fifth finger with a piece of cotton saturated with 70% alcohol. The finger should be scrubbed thoroughly, since this cleans it and also stimulates blood flow. Let the finger air-dry.
4. Hold the cleansed finger of your partner firmly between your thumb and forefinger, and using a sterile, disposable lancet, quickly make a single puncture in the tip of the finger. Discard the lancet after use. Wipe off the first drop of blood and allow a fresh drop to accumulate.
5. Pick up a clean slide, holding the edges only, and touch it to the blood so that a small drop adheres about one-half inch from the end of slide. Quickly rotate the slide so that the surface of the slide with the drop of blood is uppermost and horizontal.
6. Use a second clean, dry slide to spread the blood. Touch the spreader slide to the center of the first slide at an angle of 45°, slanting toward the drop of blood. Move the

spreader slide along the first slide until it touches the edge of the blood droplet. Allow the blood to spread along the edge of the second slide. Then move it back away from the droplet with a smooth, fairly fast motion, maintaining contact between the two slides. Continue the motion until the edge of horizontal slide is reached, leaving a thin uniform trail of blood along the length of the slide. Allow the slide to air dry. Several trials may be necessary to obtain a slide with the blood spread thinly and uniformly.



7. Stain the slide as follows. It is important that the blood smear be dry when you start and that the slide remain in the solutions for the exact times specified.
 - a) Dip the slide into methyl alcohol (#1) for 30 seconds. Remove it and shake excess liquid onto a paper towel. Do not wipe the surface of the slide!
 - b) Dip the slide into the eosinate solution (#2) and move it gently up and down slightly for 10 seconds. Remove the slide from the solution and gently shake off excess stain onto a paper towel.
 - c) Dip the slide twice into distilled water (#3) to rinse it, and shake off excess water.
 - d) Dip the slide into the polychrome stain (#4) for 10 seconds. Shake off excess.
 - e) Rinse in distilled water (#5) by dipping it twice, shake off excess water, and allow to air dry.
8. When the slide is dry, examine it with the microscope. Since the cells are small, at least 100 X magnification is necessary. The distribution of cells will probably be uneven, with large white blood cells (now stained blue and purple) more numerous at the edges of the smear.
 - a) The red blood cells are the most numerous cells on the slide. They look like small discs, uniformly stained reddish brown, with smooth, distinct margins.

- b) The white blood cells are larger, with pale blue cytoplasm and a larger purple-stained nucleus. They are classified into five types on the basis of the shape of the nucleus, the relative amount of cytoplasm, and the presence or absence of stained granules in the cytoplasm. Unusual numbers of white blood cells per volume of blood or changes in the relative numbers of different kinds of cells present can be indicative of an infection or other diseases.

Use an oil immersion lens if one is available to examine these cells. Find as many different white blood cells as you can, and make notes or sketches of their appearance. Devise a scheme for classifying the cells into categories. Compare your scheme with those of your colleagues. Can you agree upon a classification system that everyone can use?

Discussion

Consider the relationship between what you have done in this exercise and scientific methods. On the basis of your experience with blood cells, can you progress from observing and classifying to formulating an hypothesis? Did your class arrive at a consensus about a classification scheme for the white blood cells? How has the availability of an accepted classification facilitated the study of blood cells? How is the classification used in clinical diagnosis?

References on Florence Sabin

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2. Talbott, John H. 1970. "Florence Rena Sabin (1871-1953)." In A Biographical History of Medicine. New York: Grune & Stratton, pp. 1181-1183.

References on Blood Cells

1. Most textbooks of Human Physiology or Physiology and Anatomy discuss the types of blood cells.
2. Diggs, L. W.; Sturm, Dorothy; and Bell, Ann. 1975. The Morphology of Human Blood in Wright Stained Smears of Peripheral Blood and Bone Marrow. 3rd ed. 56 p. North Chicago, Illinois: Abbott Laboratories. (A thorough, well-illustrated description.)

AUDIO-VISUAL RESOURCES

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Harrison, Cynthia Ellen. Women's Movement Media: A Source Guide. 1975. New York: R. R. Bowker Co.

Learning Arts. Box 179, Wichita, KS 67201. Careers in Science Fields. (Cassette-filmstrips.)

National Audiovisual Center. National Archives and Records Service, General Services Administration, Washington, DC. A catalog of United States Government produced audio-visual materials.

Non-print Resources in Women's Educational Equity. A catalog of films, tapes, and other instructional materials. Available from the Superintendent of Documents, GPO, Washington, DC 20402. Order # 017-080-01836-5.

Wheeler, Helen Rippier. 1975. Womanhood Media Supplement: Additional Current Resources about Women. Metuchen, NJ: Scarecrow Press.

Women's Films: A Critical Guide. 1975. Indiana University Audio-Visual Center, Bloomington, Ind. 47401

Film Distributors

AIMS Aims Instructional Media Services, Inc.
626 Justin Avenue
Glendale, California 91201

AMERGA American Personnel and Guidance Association
1607 New Hampshire Avenue
Washington, D.C. 20009

BBF Billy Budd Films
235 East 57th Street
New York, New York 10022

BFA BFA Educational Media
2211 Michigan Avenue
P. O. Box 1795
Santa Monica, California 94108

CCCD Cally Curtis Company
1111 North Las Palmas
Hollywood, California 90038

CF Churchill Films
662 North Robertson Boulevard
Los Angeles, California 90069

CORF Coronet Instructional Films
65 East South Water Street
Chicago, Illinois 60601

CREMED Creative Media
820 Keo Way
Des Moines, Iowa 50309

DIBIE Dibie-Dash Productions
4949 Hollywood Boulevard Suite 208
Los Angeles, California 90027

DOCUA Document Associates, Inc.
43 Britain Street
Toronto 285, Ontario
CANADA

EBEC Encyclopedia Britannica Educational Corporation
425 North Michigan Avenue
Chicago, Illinois 60611

EDC EDC Film Distribution Center
55 Chapel Street
Newton, Massachusetts 02158

EMC EMC Corporation
Educational Materials Division
180 East Sixth Street
St. Paul, Minnesota 55101

FI Films, Inc.
1144 Wilmette
Wilmette, Illinois 60091

HERFLM Herstory Films
137 East 13th Street
New York, New York 10012

IFB International Film Bureau
332 South Michigan Avenue
Chicago, Illinois 60604

IMPACT Impact Films
144 Bleecker Street
New York, New York 10012

IU Indiana University
Audio-Visual Center
Bloomington, Indiana 47401

KUONTV KUON-TV
University Educational TV Station
Box 83111
Lincoln, Nebraska 68501

LCOA Learning Corporation of America
1350 Avenue of the Americas
New York, New York 10019

MGHT McGraw-Hill Films
1221 Avenue of the Americas
New York, New York 10020

MTVTM Motivational Media
8271 Melrose
1001 North Poinsettis Place
Los Angeles, California 90069

NEWDAY New Day Films
P. O. Box 315
Franklin Lakes, New Jersey 07417

PHENIX Phoenix Films, Inc.
470 Park Avenue South
New York, New York 10016

POLYMR Polymorph Films, Inc.
331 Newbury Street
Boston, Massachusetts 02115

RADIM Radim Films, Inc.
17 West 60th Street
New York, New York 10023

UCEMC University of California
Extension Media Center
2223 Fulton Street
Berkeley, California 94720

UCLA UCLA Instructional Media Library
405 Hilgard Avenue
Los Angeles, California 90024

XEROX Xerox Films
245 Long Hill Road
Middletown, Connecticut 06457

Recommended Films

The ABC's of Decision Making. 1974. 30 min., color. CREMED.
Management consultant Joe Batten outlines a practical
approach to making decisions.

Accomplished Woman. 1974. 25 min., color. FI. Portraits of a
variety of women who have "made it."

American Parade: We, the Women. 1974. 30 min., color. BFA
History of women's roles and oppression of women in
the U.S. Narrated by Mary Tyler Moore.

Anything They Want To Be. 1974. 7 min., color. UCEMC.
Sex-role stereotyping in elementary and high schools
with regard to intellectual and career-oriented activities.

Aristotle and the Scientific Method. 1959. 14 min., black and
white or color. CORF. Introduces Aristotle as the
originator of the "scientific method."

Assertiveness Training for Women. 1973. Part 1: 17 min.,
Part 2: 18 min.; color. AMERGA. Series of vignettes
illustrate common situations in which women's personal
rights are taken advantage of.

Children of Change. 1960. 30 min., black and white. IFB.
Depicts stresses on child and mother when mother works.

The Emerging Woman. 1974. 40 min. RADIM. Film about the
history of women in the U.S.

The Family: Life Styles of the Future. 1971. 22 min., color.
DOCUA. Margaret Mead discusses alternative life styles,
communal living groups.

Fifty-one Percent. 1972. 59 min., color. CCCD. Depicts lack
of management opportunities for women as a group.

Growing Up Female: As Six Become One. 1971. 50 min. NEWDAY.
Socialization of the American woman, as seen through a
personal look at the lives of six women.

House of Science. 1973. 15 min., color. EBEC. Animated film
shows science growing and subdividing through history.

How a Scientist Works. 1967. 15 min., black and white or
color. EBEC. Scientist demonstrates scientific method of
problem solving.

How to Make a Woman. 58 min., color. POLYMR. Adaptation of a
feminist play on problems facing women in a male-dominated
society. Excellent for provoking discussion on the women's
liberation movement.

I Love You, Goodbye. 1974. 75 min., color. LCOA. Drama of a married woman who leaves her husband and children to start a new life.

Margaret Mead's New Guinea Journal. 1969. Parts I, II, III: 90 min., color. IU. Mead narrates this film on her return to a New Guinea village 40 years after her first expedition.

Masculine or Feminine: Your Role in Society. 1971. 19 min., color. CORF. Explores roles of men and women in today's world by presenting their own feelings on the subject.

Mathematical Peep Show. 1961. 12 min., color. EPEC. Five segments that simply and clearly illustrate sophisticated mathematical concepts.

Matina Horner: Portrait of a Person. 1974. 17 min. PHENIX. This portrait of the educator and president of Radcliffe College emphasizes her theories and the programs she originated.

Men's Lives. 1974. 43 min., color. NEWDAY. Depicts social conditioning of American males. Shows how the pressures of a competitive economic system have molded American men into the traditional masculine role of aggressive, lone provider.

Mothers Are People. 1974. 8 min., color. EDC. Problems of working mothers.

Mothers: What They Do. 1968. 11 min., color. AIMS. Portrays three kinds of mothers: full-time housewife, full-time employee, part-time employee.

The Nature of Science: Forming Hypotheses. 1972. 16 min., black and white or color. CORF.

The Nature of Science: How Ideas Change. 1972. 14 min., black and white or color. CORF.

The Nature of Science: Obtaining Facts. 1972. 11 min., black and white or color. CORF.

The Nature of Science: Testing Hypotheses. 1972. 14 min., black and white or color. CORF.

Roll Over. 1974. 10 min., color. HERFLM. Depicts changing women's roles.

Scientific Method. 1954. 12 min., black and white or color. EBEC. Explains elements and applications of scientific method.

Scientific Method in Action. 1960. 19 min., color. IFB. Uses Galileo's work, and later Salk's work, to introduce the scientific method.

Silent Spring of Rachel Carson. 1963. Parts 1 and 2: 54 min., black and white. MGHT. Examines questions raised by Carson in her book, Silent Spring.

Sociobiology: Doing What Comes Naturally. 1972. 22 min., color. DOCUA. Explores research on biological origins of human behavior, including male aggression and dominance versus female passivity.

Sylvia, Fran, and Joy. 1973. 25 min., black and white. CF. Examines lifestyles of three women: a working mother, a single parent, and a housewife.

Take This Woman. 1971. 25 Min., color. FI. This NBC News production shows women who have fought to obtain non-traditional careers.

Tell Me Where It Hurts. 1974. 78 min., color. LCOA. Made-for-TV drama of housewife who decides to get a job.

They Beat the Odds. 1965. 22 min., color. DIBIE. Life stories of successful black professional women.

Three Lives. 1970. 70 min., color. IMPACT. In this film by Kate Millett, three women talk about their lives as women today.

To Be a Woman. 1970. 14 min., color. BBF. Explores femininity, sexuality, modern woman, and what constitutes womanhood.

We Are Woman. 1975. 33 min., color. MTVTM. This film employs a variety of techniques to show how women's roles have developed. Narrated by Helen Reddy.

Welthy Fisher. 1967. 30 min., black and white. IU. This film introduces the Indian educator and discusses her philosophy of education.

Woman, Wife, or What? 1960. 29 min., black and white. KUONTZ. Explores trapped feelings of many married women, the confusion over roles, and the battle for creative achievement.

Woman's Place. 1973. 58 min., color. XEROX. Traces influences that create and perpetuate women's roles. Gives examples of how women are now questioning those roles.

Women: Up the Career Ladder. 1972. 30 min. UCLA. Interviews with eight women in business, education, government.

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Evaluation of Individuals

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- Bingham, William C., and House, Elaine. 1970. Questionnaire on the Occupational Status of Women. #007496. Tests in Microfiche, A Test Collection, Educational Testing Service, Princeton, NJ 08540.
- Burns, Maire S. H. 1974. Life Styles of Women: An Attitude Scale. Department of Pysychology, Penn State University, University Park, PA.
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- Gough, Harrison. 1957. Personal Values Abstract from C.P.I. Palo Alto, CA: Consulting Psychologist Press.
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- MacDonald, A. P., Jr. Sex-Role Survey. #007496. Tests in Microfiche -- A Test Collection, Educational Testing Service, Princeton, NJ 08540.
- Mason, K. O. 1975. Sex-Role Attitude Items and Scales. U. S. Sample Surveys, National Institute for Mental Health, Washington, DC.
- Robinson, John P.; Athanasiou, R.; Head, Kendra B. 1969. Measures of Occupational Attitudes and Occupational Characteristics. Institute for Social Research, University of Michigan, Ann Arbor, MI.

- Robinson, John P., and Shaver, Phillip R. 1973. Measures of Social Psychological Attitudes. Survey Research Center, Institute for Social Research, Ann Arbor, MI.
- Rosen, Pamela (ed.). 1973. Self-Concept Measures--Grade 7 and Above. ERIC-ED083-319. Educational Testing Service, Princeton, NJ 08540.
- Spence, Janet, and Helmrich, Robert, 1972. Attitudes Toward Women Scale. Department of Psychology, University of Texas, Austin, TX 78712.
- Steinmann, Anne, and Fox, David J. 1966-68. Mafer Inventory of Feminine Values. Mafer Foundation, Inc., 124 East 28th Street, New York, NY 10016.

Evaluation of Materials

- Council on Interracial Books for Children. Checklists on Sexism. 1841 Broadway, New York, NY 10023.
- Massachusetts Department of Education. A Guide for the Evaluation of Instructional Materials [for sex and race bias]. Bureau of Equal Educational Opportunity, 182 Tremont Street, Boston, MA. 02111

Evaluation of Institutions

- Resource Center on Sex Roles in Education. Complying with Title IX, Implementing Institutional Self-Evaluation. National Fund for the Improvement of Education, Room 804E, 1201 16th Street NW, Washington, DC 20036.
- Weiss, Carol H. 1972. Evaluation Research: Methods for Assessing Program Effectiveness. Englewood Cliffs, NJ: Prentice-Hall.